PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXIX. For the Year 1789.

PART I.

LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

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VOL. LXXIX. For the Year 1789.

PART I.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the Chair, to be given to the authors of ſuch papers as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged, for the Year 1788, the Medal on Sir GODFREY COPLEY's Donation, to CHARLES BLAGDEN, M. D. Sec. R. S. for his Two Papers on Congelation, printed in the laſt Volume of the Philoſophical Tranſactions.

ERRATUM.

Page 37. line 15. *for* $\frac{175}{5}$ *read* $\frac{175}{7}$

PHILOSOPHICAL TRANSACTIONS.

1. *Description of an Improvement in the Application of the* Quadrant of Altitude *to a celestial Globe, for the Resolution of Problems dependant on* Azimuth and Altitude. *By Mr.* John Smeaton, *F. R. S.; communicated by Mr.* William Wales, *F. R. S.*

Read November 20, 1788.

PERHAPS there are few instruments that better fulfil their design in general, or more naturally represent the movements they are intended to explain and illustrate, than the terrestrial and celestial globe, which are also applied to resolve some of the problems of the sphere, which they most readily do. I believe, however, that whoever applies to

 them

them for the laſt mentioned purpoſe, will find them more defective in ſome reſpects than they are in others.

The difficulty that has occurred in fixing a ſemicircle, ſo as to have a center in the *zenith* and *nadir* points of the globe, at the ſame time that the meridian is left at liberty to raiſe the pole to its deſired elevation, I ſuppoſe, has induced the globe-makers to be contented with the *ſtrip* of thin flexible braſs, called the *quadrant of altitude*; and it is well known how imperfectly it performs its office.

The improvement I have attempted, is in the application of a *quadrant of altitude*, of a more ſolid conſtruction; which being affixed to a braſs ſocket of ſome length, and this ground, and made to turn upon an upright ſteel ſpindle, fixed in the zenith, ſteadily directs the *quadrant*, or rather *arc*, of *altitude* to its true *azimuth*, without being at liberty to deviate from a vertical circle to the right hand or left: by which means the azimuth and altitude are given with the ſame exactneſs as the meaſure of any other of the great circles.

With reſpect to the horary circle, as the common application ſeems very convenient on account of the ready adjuſtment of its index to anſwer the culmination of any of the heavenly bodies; and as I find that a circle of four inches diameter is capable of an actual and very diſtinguiſhable diviſion into 720 parts, anſwerable to two minutes of time each, which may ſerve a globe of the largeſt ſize; it ſeems that it ſhould rather be *improved* than omitted; and, if inſtead of a *pointer*, an index *ſtroke* is uſed in the ſame plane with that of the diviſions, the ſingle minutes, and even half minutes, may be readily diſtinguiſhed.

This globe, though mounted merely as a model for experiment, and only nine inches in diameter, appears capable of bringing out the ſolution to a quarter of a degree; which, I appre-

apprehend, may be eſteemed ſufficient not only as a check upon numerical computation, but to come near enough to find ſtars in the day-time in the field of teleſcopes, which, having no equatorial motion, are only capable of direction in altitude and azimuth; but from globes of a larger ſize, we may expect to come proportionably nearer.

Explanation of the figures, Plate I.

The figures 1. and 2. being different views of the ſame things, AB repreſents a line, in common to both, in the ſurface of the horizon, which here is of braſs.

CD, CD, are vertical lines, ſuppoſed to paſs through the center of the globe in each figure; and

EFG, EFG, are portions of great circles of the globe.

Fig. 1. ſuppoſes the ſpectator looking at the apparatus of the globe from the ſouth point of the horizon; therefore the circular arch EFG, in this poſition, will be a part of the *prime vertical*, and the ſmall paralellogram HI is ſuppoſed to be a *ſection* of the braſs meridian, according to that vertical plane.

Fig. 2. is a view of the ſame parts, the ſpectator being ſuppoſed to look at them from the weſt point of the horizon; and in this poſition HI is ſuppoſed to be a *portion* of the *braſs meridian*. This being fixed in mind, in what follows the ſame letters denote the ſame parts in both figures.—KLM denotes a piece of braſs, or braſs carriage, made to fit upon the vertical part of the meridian, and capable of ſliding 5° on each ſide of that point, ſo as to adjuſt to it, and to fix faſt there, by means of the finger ſcrew N *. This piece of braſs carries

* The holes repreſented in the portion of the braſs meridian (HI, fig. 2.) are ſcrew holes at five degrees diſtance, in this quarter of the circle, into any of which the finger ſcrew N is to be put as occaſion may require; the *ſlit* allowing ſufficiently for adjuſtment.

the *ſteel ſpindle* PQ, which is firmly ſocketed into it at K, according to the dotted lines *o r*, *o r*. The axis of this ſpindle is therefore capable of being ſet upright upon the *zenith point*, and to maintain that poſition with a ſufficient degree of firmneſs.—R*q*, R*q*, repreſents the ſection of a braſs ſocket made to fit the ſpindle, and turn round freely upon it; and when home to the ſhoulder at *o o*, to turn without ſhake; the ſocket and ſpindle being a ſmall matter taper, and *ground* together. On one ſide of the ſocket is firmly fixed the arm ST, by ſcrews or ſolder.—UW is an arch of 80 degrees, ſerving inſtead of the quadrant of altitude, and of the ſame *ſubſtance* as the meridian. This is firmly ſcrewed to the arm, and adjuſted by conſtruction, ſo that when the *ſpindle* is vertical, the face of this arch ſhall make part of a vertical circle.—This arch being a portion of a circle, of the ſame diameter as the braſs meridian, when its point *zero* at W reſts upon the braſs horizon, its inſide ſurface is made to agree with that of the horizon by means of a ſmall thin *nib* of braſs; that being attached to the inſide of the bottom of the quadrant of altitude at W, and projecting a little below it, gently bears againſt the inſide of the horizon, in ſubſtance occupying about half the *clearance* between the body of the globe and its ſurrounding horizon: this *nib*, ſeen edgeways, is ſhewn at the letter X. By this means the altitude of the object is ſhewn upon the working face of the quadrant, and the quadrant's bottom ſhews the azimuth upon the horizon; at the ſame time the globe is free to revolve upon its axis, clear of all the circles.

The quadrant might be made complete to 90°; but as in theſe middle latitudes there is very little buſineſs for azimuths when the altitudes are above 80°, and as I judged it eligible, that the quadrant ſhould be made capable of working on both ſides the meridian; *that* would be prevented by the neceſſary

thickneſs that the circles require to give them ſolidity, in contradiſtinction to *mathematical planes*; unleſs a part of a quadrant was cut out next the vertex to give them clearance: by this means the arch being lifted up from the ſpindle, and put on the other ſide of the braſs meridian for the afternoon, it will then come within 10° or 15° of the meridian; and if the uſe of this ſpace ſhould be wanted, it can be ſupplied by reverſing the ſimilar operation for the morning; and the back ſide of the upper end of the quadrant at U being champered, or *bevilled* off, this will admit it to come as near to the meridian as I have mentioned.

The ſteel ſpindle is eaſily adjuſted to the *zenith*; for the globe being rectified to its *latitude*, ſet the braſs carriage at liberty, bring the quadrant and meridian together, face to face, and ſlide the carriage, till the lower extremity of the quadrant *buts* upon the horizon, and there ſcrew it faſt.

It is, however, to be noted, that I have found ſomething neceſſary by way of *holdfaſt*, to prevent the braſs meridian from ſhifting its latitude, and that without confining it in any other reſpect.—What I have found to anſwer this purpoſe is repreſented, fig. 3. The crutch-like piece of wood ABC is ſhewn as ſeen looking right down upon it. The circle DE is the horizontal ſection of the ſouth pillar of the globe. The ſtrong wire pin FG, that goes through the two arms of the crutch and pillar, ſerves as an axis upon which its other extremity at B is at free liberty to lift up and down, but without ſhake upon the pin; and the whole being ſplit with a fine ſaw, from B to H, the notch BK lays hold of the under ſide of the braſs meridian, and by tightening the finger ſcrew LM, it firmly clips it, and retains it in any given poſition. And that it may be under no confinement *croſs-ways*, the hole in the pillar is opened on both ſides,

as

as ſhewn in the ſection, to give it liberty of accommodation; the pin being faſt in the two ends of the crutch, and turning gently in the pillar; the whole being ſlender and compliant, except in point of length.

N. B. Thoſe that would uſe the globe to the beſt advantage to ſolve problems, ſhould be careful to get a *juſt* declination, as alſo a *diſtinct* point to mark it; and as the circles and diviſions upon the ſurface of the globe itſelf, are not always ſufficiently to be depended on for this purpoſe, I have found the following expedient fully to anſwer. Chuſe any plain white part of the globe's ſurface, anſwerable to the declination given, and with the point of a needle or protracting pin, by the help of the diviſions of the braſs meridian, mark a fine point upon the blank ſurface of the globe, and upon this point make a dot with ink, with the ſmall point of a pen, which rub off with the finger, and it will leave a fine black ſpeck behind. This dot being brought to the meridian, rectify the horary index to it, and it will accurately repreſent the center of the celeſtial body whoſe inveſtigation is wanted.

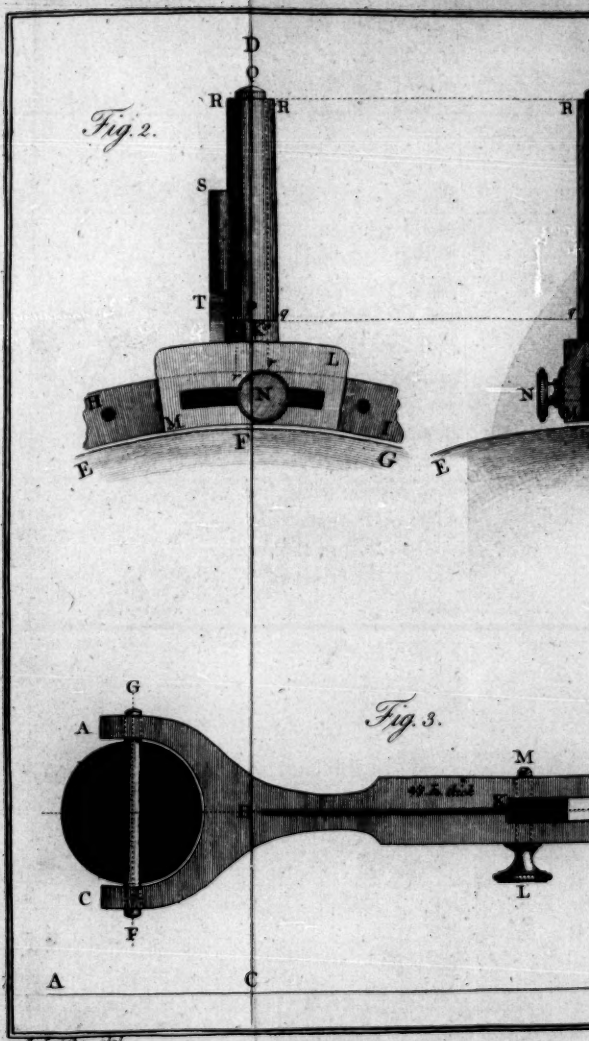
Fig. 2.
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Fig. 3.
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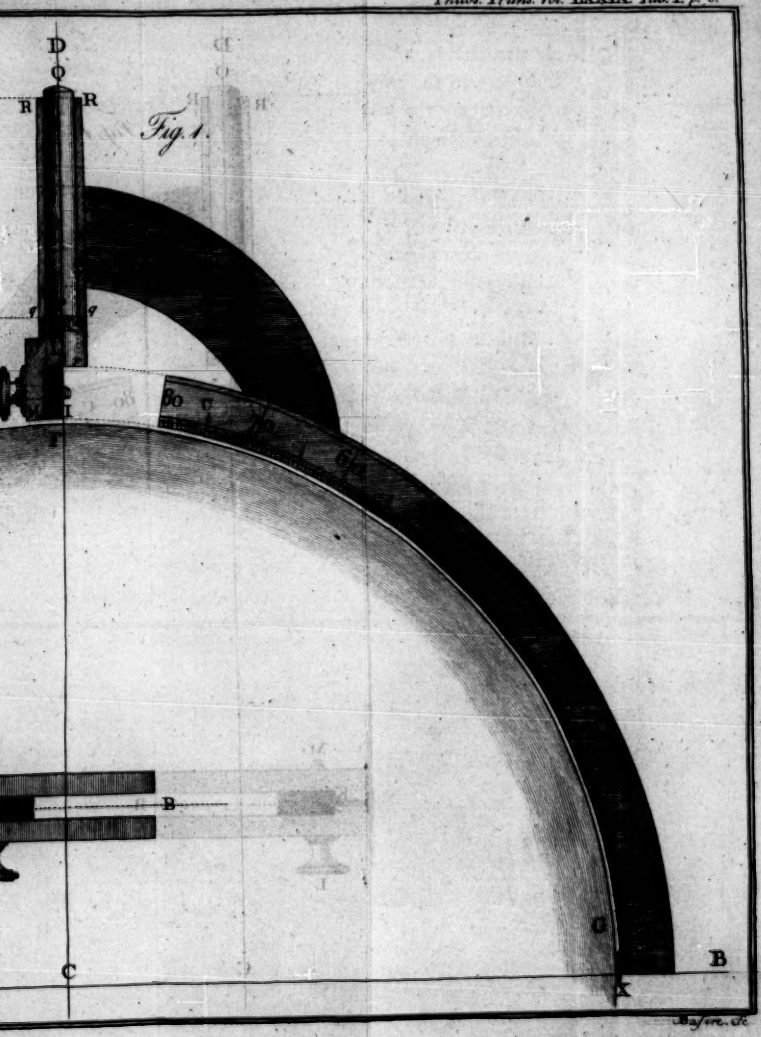

Fig. 1.

Basire. Sc.

II. *Objections to the Experiments and Obſervations relating to the Principle of Acidity, the Compoſition of Water, and Phlogiſton, conſidered; with farther Experiments and Obſervations on the ſame Subject.* *By the Rev.* Joſeph Prieſtley, *LL.D. F. R. S.*

Read November 27, 1788.

HAVING never failed, when the experiments were conducted with due attention, to procure ſome *acid* whenever I decompoſed dephlogiſticated and inflammable air in cloſe veſſels, I concluded that an acid was the neceſſary reſult of the union of thoſe two kinds of air, and not water only; which is an hypotheſis that has been maintained by Mr. LAVOISIER and others, and which has been made the baſis of an intirely new ſyſtem of chemiſtry, to which a new ſyſtem of terms and characters has been adapted. The *facts* that I alleged were not diſputed; but to my *concluſion* it was objected, that the acid I procured might come from the phlogiſticated air, which in one of my proceſſes could not be excluded; and that it was reaſonable to conclude that this was the caſe, becauſe Mr. CAVENDISH had procured the ſame acid, *viz.* the nitrous, by decompoſing dephlogiſticated and phlogiſticated air with the electric ſpark. In other caſes it has been ſaid, that the *fixed air* I procured came from the *plumbago* in the iron from which my inflammable air had been extracted.

With reſpect to the former of theſe objections I would obſerve, that my proceſs is very different from that of Mr. CAVENDISH;

CAVENDISH; his decompoſition being a very ſlow one by electricity, and mine a very rapid one by *ſimple ignition*, a proceſs by which phlogiſticated air, as I found by actual trial, was not at all affected; the dephlogiſticated and inflammable airs uniting, and leaving the phlogiſticated air (as they probably would any other kind of air with which they might have been mixed) juſt as it was.

I would alſo obſerve, that there is no contradiction whatever between Mr. CAVENDISH's experiment and mine, ſince phlogiſticated air may contain phlogiſton, and by means of electricity this principle may be evolved, and unite with the dephlogiſticated air (or with the acid principle contained in it) as in the proceſs of ſimple ignition the ſame principle is evolved from inflammable air, in order to form the ſame union; in conſequence of which, the water, which was a neceſſary ingredient in the compoſition of both the kinds of air, is precipitated. That in other circumſtances than thoſe in which I made the experiments, the acid wholly eſcaped, and nothing but water was found, may be eaſily accounted for, from the ſmall quantity of the acid principle in proportion to the water, and the extreme volatility of it, owing, I preſume, to its high phlogiſtication when formed in this manner.

In order to aſcertain the effect of the preſence of phlogiſticated air in this proceſs, I now not only repeated the experiment of mixing a given quantity of phlogiſticated air with the two other kinds of air, and found, as before, that it was not affected by the operation; but I made the experiment with atmoſpheric air, inſtead of dephlogiſticated. Since the air of the atmoſphere contains a greater proportion of phlogiſticated air, it might be expected that, if the acid I got before came from the ſmall quantity of phlogiſticated air which I could not

not poſſibly exclude, I ſhould certainly get more acid, when, inſtead of endeavouring to exclude it, I purpoſely introduced a greater quantity. But the conſequence was the production of much leſs acid than before, the liquor I procured being ſometimes not to be diſtinguiſhed from pure water, except by the greateſt attention poſſible: for though the decompoſition was made in the ſame copper veſſel which I uſed in the former experiments, there was now no ſenſible tinge of green colour in it.

When I repeated this experiment in a glaſs veſſel, I perceived, as I imagined, the reaſon of the ſmall produce of acid in theſe new circumſtances: for the veſſel was filled with a vapour which was not ſoon condenſed, and being diffuſed through the phlogiſticated air (which is not affected by the proceſs) is drawn away along with it, when the exhauſting of the tube is repeated; whereas, when there is little or no air in the veſſel beſides the two kinds which unite with each other, and are decompoſed, the acid vapour, having nothing to attach itſelf to and ſupport it (by being entangled with it) much ſooner attacks the copper, making the deep green liquor which I have deſcribed. Sometimes, however, I have procured a liquor which was ſenſibly green by the decompoſition of atmoſpheric and inflammable air, but by no means of ſo deep a colour, or ſo ſenſibly acid, as when the dephlogiſticated air is uſed.

The extreme volatility of the acid thus formed (and which accounts for the eſcape of ſome part of it in all theſe proceſſes) is apparent from this circumſtance, that if the exploſions be made in quick ſucceſſion (the tube being exhauſted immediately after each of them, and filled again as ſoon as poſſible) no liquor at all will be collected, the whole of the

acid vapour, together with the water with which it was combined, being drawn off uncondenſed in every proceſs. I once made twenty ſucceſſive exploſions of this kind, in a copper tube, out of which I found that I drew 37 ounce meaſures of air by the action of the pump, and found not a ſingle drop of liquid, though near an hour was employed in the whole proceſs, and the veſſel was never made more than a little warmer than my hand. This was a degree of heat by no means ſufficient to keep the whole of any quantity of water in a ſtate of vapour; and is a circumſtance that of itſelf ſufficiently proves, that the vapour did not conſiſt of water only.

Indeed, I think it impoſſible for any one to *ſee* this vapour in a tall glaſs veſſel, and eſpecially to obſerve how it falls from one end of it to the other, and the time that is required to its wholly diſappearing, without being ſatisfied that it conſiſts of ſomething elſe than mere water, the vapour of which would be more equally diffuſed. If the appearance to the eye ſhould fail to convince any perſon of this, the ſenſe of *ſmell* would do it: for even in a glaſs veſſel it is very offenſive, though it might not be pronounced to be *acid*. I conjecture, however, that this, and every other ſpecies of *ſmell*, is produced by ſome modification of the acid or alkaline principle. Some may be diſpoſed to aſcribe this ſmell to the *iron* from which the inflammable air was produced; but the ſmell is the ſame, or nearly ſo, when the air is from tin, and would probably be the ſame if it were from any other ſubſtance.

Beſides uſing atmoſpheric air, which contains a greater proportion of phlogiſticated air, I have ſometimes uſed dephlogiſticated air which was not very pure; and in this caſe I have always obſerved, that the liquor I procured had leſs colour, and was leſs ſenſibly acid.

Theſe obſervations might, I ſhould think, ſatisfy any reaſonable perſon, that the acid liquor which I procured by the exploſion of dephlogiſticated and inflammable air in cloſe veſſels did not come from the phlogiſticated air which could not be excluded, whether it was that which remained in the veſſel after exhauſting it by the air pump, or that with which the dephlogiſticated air was more or leſs contaminated.

But beſides theſe experiments, in which I procured the green acid liquor by the exploſion of dephlogiſticated and inflammable air in cloſe veſſels, I made another, to which I thought the ſame objection could not have been made, becauſe no air pump was uſed in it, and nothing but the pureſt dephlogiſticated air was employed, being ſeparated in the proceſs from *precipitate per ſe* in contact with the pureſt inflammable air in a glaſs veſſel which had been previouſly filled with mercury. Accordingly, the only objection made to *this* experiment was, that the preparation I made uſe of might be impure, containing ſomething which might yield phlogiſticated air. This appeared to me highly improbable, as the precipitate had been made by M. Cadet, and for the purpoſe of philoſophical experiments. Beſides, if the heat of a burning lens ſhould diſlodge phlogiſticated air from any unperceived impurity in this preparation, mere *heat* will not decompoſe this air. Let any perſon try the effect of a lens on ſuch air, or any ſubſtance containing it, and produce an acid if he can.

M. Bertholet, however, thinking that this might be the caſe, deſired that I would ſend him a ſpecimen of my precipitate *per ſe*. Accordingly, I ſent him all that remained of it; and, in return, he ſent me a quantity on the goodneſs of which I might depend. With this preparation I repeated my former experiment; and, by giving more attention to the

proceſs, found it to be far more deciſively concluſive in favour of my opinion than I had imagined. In the former experiment I had attended only to the drop of *water* which was found in the veſſel in which the proceſs was made; and finding that it turned the juice of turnſole red, I concluded, that it contained nitrous acid: but I now examined the *air* that remained in the veſſel, and found that a conſiderable proportion of it was fixed air; ſo that I am now ſatisfied *this* was the acid with which it was impregnated, and not the *nitrous*. Still, however, ſome acid is the conſtant reſult of the union of the two kinds of air, and not water only. A quantity of the ſame precipitate *per ſe* yielded no fixed air by heat.

Comparing this experiment with that in which iron is ignited in dephlogiſticated air, this general concluſion may be drawn, *viz.* that when either inflammable or dephlogiſticated air is extracted from any ſubſtance in contact with the other kind of air, ſo that one of them is made to unite with the other in what may be called its *naſcent ſtate*, the reſult will be *fixed* air; but that if both of them be completely formed before their union, the reſult will be *nitrous acid*.

It has been ſaid, that the fixed air produced in both theſe experiments may come from the *plumbago* in the iron from which the inflammable air is obtained. But ſince we aſcertain the quantity of plumbago contained in iron by what remains after its ſolution in acids, it is in the higheſt degree improbable, that whatever plumbago there may be in iron, any part of it ſhould enter into the inflammable air procured from it. Beſides, according to the antiphlogiſtic hypotheſis, all inflammable air comes from water only.

As it cannot be ſaid, that any real fixed air is found in inflammable air from iron (ſince it is not diſcoverable by lime-

water) it muſt be ſuppoſed, that the elements, or component parts of fixed air are in it; but one of theſe elements is pure air, and the mixture of nitrous air ſhews, that it contains no ſuch thing, though, according to M. LAVOISIER, fixed air contains 72 parts in 100 of pure air.

However, being apprized of this objection to inflammable air from iron, I made uſe of inflammable air from *tin*, and I had the ſame reſult as with that from iron. I alſo calculated the weight of the fixed air which I got in the proceſs, and comparing it with the plumbago which the iron neceſſary to make the inflammable could have contained, I found, that, in all the caſes, it far exceeded the weight of the plumbago; ſo that it was abſolutely impoſſible, that the fixed air which I found ſhould have had this origin. For the greater ſatisfaction, I ſhall recite the particulars of a few experiments of this kind.

In ten ounce meaſures of inflammable air from malleable iron I revived *red precipitate* till there remained only 1.1 oz. meaſure of air, and of this 0.07 oz. m. was fixed air, being completely abſorbed by water. The weight of this air would be 0.063 gr. But, ſince 960 grains of iron will yield 1054 oz. meaſures of inflammable air, the iron employed in procuring all the inflammable air that was uſed in this experiment, *viz.* 8.9 oz. meaſures (without allowing for any that went to the revivification of the mercury) would be 8.1 grains; and ſince M. BERGMAN ſuppoſes, that 100 grains of iron contains 0.12 gr. of plumbago, the quantity of it in this iron would only be 0.01008 gr. which is not quite a ſixth part of the weight of the fixed air.

With the *precipitate per ſe*, ſent me by M. BERTHOLLET, I revived mercury till 8¼ oz. m. of inflammable air was reduced to 2¼ oz. m., and of this 0.04 oz. m. at leaſt was fixed air.

This

This is not quite ſo much in proportion as in the preceding experiment, but abundantly more than the weight of the plumbago.

In 8 oz. m. of inflammable air I revived *minium* (which I found to have exactly the ſame effect in this proceſs as red precipitate, or precipitate *per ſe*), till it was reduced to 1.2 oz. m.; and of this 0.028 oz. m. was fixed air, which would exceed the weight of the plumbago more than three times. In reviving lead from maſſicot (which I prepared by expelling the pure air from minium) I had no fixed air in the reſiduum.

In 7 oz. m. of inflammable air from tin by ſpirit of ſalt, I revived red precipitate till it was reduced to 1.1 oz. m.; and in this the fixed air was ſomething more than in proportion to that in the laſt experiment.

In my laſt volume of Experiments, p. 30. I mentioned ſome inſtances of the revival of red precipitate in inflammable air, without finding any fixed air, though in one I perceived a ſlight appearance of it. To this I can only ſay, that I now always find it, and have, in the preceding caſes, meaſured the quantity of it; ſo that, though I did not find any before, I muſt preſume that I did not uſe the ſame precautions that I did at this time: and it is poſſible, that I might not attend to the effect of admitting a large quantity of water to a ſmall quantity of fixed air, which would preſently abſorb the greateſt part of it. I alſo think I recollect, that I then continued the proceſs as far as I poſſibly could, and conſequently left very little air in the veſſel; whereas I now purpoſely left a good deal, that the admiſſion of water might have leſs effect on the fixed air diffuſed through it.

This alſo may be ſaid in favour of the greater accuracy of my preſent experiments, that they intirely remove a very great difficulty,

difficulty, which I acknowledged, p. 128. in finding different reſults from ſeemingly ſimilar circumſtances; whereas I now find that both the circumſtances and the reſults are different. Beſides, the *poſitive* evidence of actually finding a ſubſtance is always more concluſive than the *negative* one, of not finding it.

I do not know that any objection can be made to the inflammable air from *tin*, as this metal has not been proved to contain plumbago. I wiſhed, however, to repeat this experiment with inflammable air from *ſulphur*. But though, when ſteam is ſent over melted ſulphur, a ſmall quantity of inflammable air is procured, as I obſerved in my laſt volume of experiments; yet, as ſulphur cannot part with much phlogiſton, except in proportion as it imbibes pure air, to form oil of vitriol, I could not in this manner eaſily procure enough for my purpoſe.

In order to ſupply the ſulphur with pure air, I mixed with it a quantity of *turbith mineral*; but this made it yield vitriolic acid air, though in great abundance, there not being, I imagine, *water* enough to form inflammable air: for when iron is diſſolved in concentrated acid of vitriol, vitriolic acid air is produced; but in diluted vitriolic acid, the produce is inflammable air. With a view to ſupply theſe materials with water, I ſent ſteam over them; but it did not combine with the air, which was ſtill only vitriolic acid air.

Since, however, vitriolic acid air unqueſtionably contains the ſame principle which forms the inflammability of inflammable air, this experiment proves, that ſulphur is not that ſimple ſubſtance which the antiphlogiſtians ſuppoſe it to be; but that it contains phlogiſton. Had it been nothing more than a ſubſtance which had a ſtrong affinity to pure air, it would

have

have united with the pure air from the turbith mineral, and have made vitriolic acid; but no vitriolic acid air would have been produced.

That vitriolic acid air contains the ſame inflammable principle with inflammable air is evident from the quantity of vitriolic acid air which I produced by reviving copper from blue vitriol in inflammable air. See my Experiments, vol. VI. p. 15. Mr. KIRWAN alſo produced this air from ſulphur and red precipitate. See his Treatiſe on Phlogiſton, p. 29.

When I uſed a ſmall quantity of ſulphur in proportion to the turbith mineral, the firſt produce was vitriolic acid air, and afterwards dephlogiſticated air, from the turbith mineral alone, the effect of the ſulphur having been exhauſted.

According to the antiphlogiſtic theory, *phoſphorus*, as well as ſulphur, is a ſimple ſubſtance; and when it is ignited imbibes pure air, and thereby becomes the phoſphoric acid, without parting with any thing. But I find, that after the accenſion of it in dephlogiſticated air, there is a conſiderable quantity of fixed air in the reſiduum; and this fixed air could only be formed by the union of the dephlogiſticated air in the veſſel with the phlogiſton contained in the phoſphorus. Mr. KIRWAN had a ſimilar reſult from phoſphorus confined in atmoſpheric air. As it is not pretended, that there is any plumbago in phoſphorus, this experiment is not liable to the objection that has been made to thoſe in which inflammable air from iron was made uſe of.

It will be expected, that in this reply to the objections that have been made to my experiments eſtabliſhing the doctrine of phlogiſton, I ſhould conſider what has been alledged by Meſſ. LAVOISIER, BERTHOLLET, and DE FOURCROY, in favour of their new ſyſtem, in their *Report* on the ſubject of the new chemical

chemical characters invented by Meſſ. Hassenratz and Adet, ſubjoined to the new *Nomenclature Chymique*. I ſhall therefore notice what appears to me to be moſt important in that publication.

"One of the articles of the modern doctrine" (of which they ſay, p. 311. "that it coſt more than twenty years labour, which "the force of reaſoning has obliged many celebrated chemiſts to "adopt, and in favour of which much greater numbers are ready "to decide;" and the evidence for which they ſay, p. 301. "is the moſt complete chemical proof), which ſeems the "moſt ſolidly eſtabliſhed," p. 298, "is the formation, the "decompoſition, and recompoſition of water; and how is "it poſſible," they add, "to doubt of it, when we ſee that, "in burning together 15 grains of inflammable air and 85 of "pure air, we get exactly 100 grains of water; and when we "can, by decompoſition, find again theſe ſame two principles, "in the ſame proportions?"

To this I muſt ſay, as I have done, Experiments, vol. VI. p. 139. (and when I wrote that, I was myſelf a believer in the decompoſition of water), that I have never been able to find the full weight of the air decompoſed in the water produced by the decompoſition; and that now I apprehend it will not be denied, that the produce of this decompoſition is not mere water, but always ſome acid.

As to the ſuppoſed decompoſition of water by means of iron, I have ſhewn that it is a fallacy; ſince the iron imbibes nothing but water when it parts with its phlogiſton. And I have obſerved (Experiments, vol. VI. p. 83.), that when this finery cinder is reconverted into iron by inflammable air, nothing but water is expelled from it; and that the reſiduum of the air is purely inflammable, without containing any fixed

air. It is evident, therefore, that the iron had imbibed pure water only. Had the iron imbibed dephlogisticated air from the water, and not water itself, there seems to be no reason why fixed air should not be found in this, as well as in the exactly similar process with minium and precipitate *per se*. Also, it can never be supposed, that the addition which iron gains, of one-third of its weight, is from air contained in steam, if it could be proved to contain any; because, if there be a sufficient quantity of iron, the whole of the water will be imbibed; so that, on this hypothesis, water must be nothing but dephlogisticated air condensed.

There is, I acknowledge, a great difficulty in explaining the experiment of iron first imbibing water, and parting with phlogiston, and again parting with its water, and imbibing phlogiston, in circumstances of heat so nearly similar as those which I have described. It seems as if the affinity of iron to water and to phlogiston was each, in their turns, stronger than the other. To this I can only say, that the whole doctrine of affinities, as far as it is true, is founded on facts; and these are clearly such as I have represented; and that a difference of circumstances, which is not apparent at present, may become so when we shall have given sufficient attention to them.

In order to satisfy myself whether any thing besides *water* was expelled from finery cinder by heat, I went through similar processes with this substance and *massicot*, from which all air had been previously expelled; and after reviving both of them in inflammable air, I found the results, in all respects, the very same. The residuums of the inflammable air were equally free from fixed air; and when they were fired with equal quantities of dephlogisticated air, the diminutions of

bulk

bulk were very nearly the same, less than when the original inflammable air was used, because all the impurities in the whole quantity were retained in a small residuum, the metals having imbibed nothing but pure phlogiston. Also the inflammable air had been long confined by water, in consequence of which it is always altered more or less. The particulars of the processes were as follows:

The finery cinder was revived in 7 oz. m. of inflammable air, which was thereby reduced to 1½ oz. m.; and an oz. m. of this residuum being fired together with an equal quantity of dephlogisticated air, not very pure, the diminution of both was to 28 divisions of a tube, of which 30 was one oz. m. when with equal quantities of the same dephlogisticated and the original inflammable air the diminution was to 18.

The massicot was reduced in 8 oz. m. of inflammable air till it was reduced to 1½ oz. m.; and after the process with the dephlogisticated air, the diminution was to 29, when with the original inflammable air it was to 174.

In both the residuums, after the explosion, there was a slight appearance of *fixed air*, though none could be perceived before the explosion; but in both cases it was so slight that it could not have been perceived by the diminution of its bulk. But since both fixed air and nitrous acid are produced from the same materials in different circumstances, it cannot be thought extraordinary if, in some cases, both should be produced at the same time.

M. Lavoisier and his associates further observe, p. 300, with respect to my experiments, that "when a calx is revived "in inflammable air, more water is found in the vessel than the "weight of inflammable air that disappears, so that it could "not have been contained in that air." They only refer to

my experiments in general; but as they ſpeak of the water produced as appearing both on the inſide of the veſſel, and on the ſurface of the mercury, it can be no other than the experiment of the revival of iron from finery cinder; and the water that is found in this proceſs was never ſuppoſed to come from the little that is contained in the inflammable air, but the much greater quantity contained in the cinder.

Before I conclude this Paper, I ſhall juſt mention a few circumſtances attending the many exploſions I have made of inflammable and dephlogiſticated air in the long metallic and glaſs veſſels I have made uſe of, as they were pretty remarkable. The exploſions were made by a ſmall electric ſpark at one end of the veſſel, and the greateſt force of the exploſion was always at the other end. No tinned iron veſſel could bear many of them before they ſwelled out at that end, and at length burſt; and even the flat end of the copper veſſel, which was not leſs than one-tenth of an inch thick, was in time made quite convex, and the cylindrical part next to it was made very ſenſibly wider than any other part of the tube. This muſt have been effected by mere *force*, and not by *heat*; for the hotteſt part of the tube, after every exploſion, was never there, but always about the middle, though ſomething nearer to that end than the other, and in the glaſs veſſel the denſe cloud was always formed at that end.

The probability is, that the air where the electric ſpark is made taking fire firſt, the inflammation does not extend itſelf ſo rapidly but that the air at the oppoſite end is firſt condenſed, in conſequence of the inflammation and expanſion of the air at the other end, ſo that the air is there fired in a condenſed ſtate; and hence its greater force.

III. *Observations on the Class of Animals called, by* Linnæus, *Amphibia; particularly on the Means of distinguishing those Serpents which are venomous, from those which are not so.* *By* Edward Whitaker Gray, *M. D. F. R. S.*

Read December 18, 1788.

OF the various classes of the animal kingdom, no one has been so little attended to as the class, called by Linnæus, Amphibia. What he himself did in that class (though far superior to what any other person has done) was evidently done in a hurry; false references are, at least, as common in that, as in any other part of his works, and many of his descriptions are given in a very careless manner; others there are, however, which are truly worthy of their author, and in which the specific characters are pointed out with that clearness and precision, which so eminently distinguish the descriptions of Linnæus from those of all his predecessors.

In the construction of the class, Linnæus has been particularly unfortunate; as he has erred, not only in making an unilocular heart one of the characters of it, but also in making the cartilaginous fishes a part of it. I think it needless to mention the causes which led him to this latter error; every anatomist now agrees that the Amphibia Nantes are not furnished with lungs; and every naturalist is convinced of the propriety of removing them, from the class of Amphibia, to that of Fishes. I shall only observe that, by the removal, the name of the class

claſs (which ſome naturaliſts have cavilled at) becomes much leſs objectionable; there being few genera, in the two orders of which it is now preſumed to conſiſt, which do not contain animals to which the term amphibious may, with ſome propriety, be given; whereas, in the order of Nantes, not one ſpecies occurs which has the ſmalleſt claim to that title. With reſpect to the other error I noticed (*viz.* that of ſuppoſing the hearts of the Amphibia to be ſingle) it would be eaſy to ſhew that it was not an uncommon one, at the time LINNÆUS formed his ſyſtem. And indeed he appears to have been led into it, by following an author whom he probably ſuppoſed of too great fame not to be ſafely relied on. At leaſt, in defence of his opinion, he quotes the following words of BOERHAAVE. "*In omnibus animalibus in quibus ſanguis non calet, ven-*"*triculus cordis eſt unicus.*" Whether the hearts of all the different genera, of which the claſs is compoſed, have yet been accurately examined; and whether an exact ſimilarity of ſtructure is found throughout the claſs; are queſtions I do not mean, at preſent, to examine. It is ſufficient for my purpoſe to obſerve, that the hearts of moſt of the Amphibia are now well known to be double, with an immediate communication between the two cavities; which ſtructure ſeems peculiarly adapted to that change of element, which (as I before obſerved) many of them can, for a time, ſupport; and thereby furniſhes another argument in favour of the name LINNÆUS has given to the claſs.

To conſider the ſtructure of the heart, however, is not abſolutely neceſſary in forming the characters of the claſs: the animals of which it conſiſts being ſufficiently diſtinguiſhed from all others, by having cold red blood, and breathing by means of lungs. Theſe two characters render the claſs perfectly diſtinct from the reſt; the two ſuperior ones, *viz.*

Mammalia and Birds, having warm blood; and the three inferior ones, *viz.* Fishes, Insects, and Worms, not being furnished with lungs.

In his generic characters, LINNÆUS has been more successful than in those of the class; insomuch that they may, I think, be considered as the best hitherto given. Whoever will be at the pains of comparing LINNÆUS's genera of Amphibia with those of GRONOVIUS, will find, that the generic characters of the former, though few in number, are precise and distinct; while those of the latter, though more numerous, are vague, indistinct, and sometimes inaccurate. As a glaring instance of inaccuracy, I need only refer to the Chamæleon, which by GRONOVIUS is made a distinct genus, of which one of his characters is, *Pedes unguibus destituti*; whereas, in fact, the feet of that animal are furnished with very distinct, and pretty large, claws.

But though LINNÆUS's genera of Amphibia are, upon the whole, well formed, it must be allowed to be a great imperfection in them, that the venomous serpents are not separated from the others.

From some expressions of his, in the Preface to the *Museum Regis*, and in the Introduction to the Class Amphibia, in the *Systema Naturæ*, it seems, that he thought it not easy to distinguish them, by any external characters; and his ideas respecting the venomous fangs themselves were (as we shall see hereafter) so vague and confused, that it was hardly possible for him to attempt to found a generic distinction upon them *.

* As a sort of comparative excuse for LINNÆUS, it may be observed, that GRONOVIUS (though he made two more genera of Serpents than LINNÆUS) did not separate the venomous ones; neither has he distinguished them by a mark (as LINNÆUS has) or by any other means.

Whether

Whether venomous Serpents can be, with certainty, distinguished from others, and if so, how they are to be known, is what I mean to consider in this Paper; in doing which I shall examine, first, how far they may be distinguished by any external characters; secondly, supposing the venomous fangs to be the only certain criterion, how those fangs are to be distinguished from common teeth.

Though Serpents, by their internal organization, naturally belong to the third class of the animal kingdom, they are, in their external form, more simple than most of the animals belonging to the three inferior classes; their external characters must consequently be very few. I shall first examine those of the head; and, as all venomous Serpents (so far as our present experience extends) are contained in the three first of LINNÆUS's genera, I shall, at present, consider only those three.

In the first genus, Crotalus, the head is broader than the neck, depressed or flat at top, and covered with small scales. These three characters are particularly observable in the three intermediate species horridus, Dryinas, and Durissus. In the miliarius the scales of the head are rather larger than in the others. The mutus I have never seen; but it certainly should not be placed among the Crotali *.

As all the species of this genus are venomous, one is naturally led, by the examination of it, to consider the forementioned characters as being, in some measure, proper to venomous serpents. In order to see how far they are so, I shall, for the present, pass over the next genus, Boa, and consider

* LINNÆUS's reason for not placing it among the Boæ seems to have been, that he supposed none of them were venomous. He appears, however, to have had his doubts about the contortrix. I have examined it, and am convinced it is venomous.

that

that which follows it, Coluber. In that genus are many venomous ſpecies, and it is very certain that, in general, they have the fore-mentioned characters; examples of which may be ſeen in the Atropos, Ceraſtes *, atrox, Berus, and others. It is, however, equally certain, that there are ſome in which they are not to be found. As an example of this, I need only mention the Naja, a ſpecies well known to be very venomous; the head of which is neither depreſſed nor broad, is covered with large ſcales, and is, in every reſpect, a complete exception to what has been ſaid, reſpecting the heads of venomous Serpents.

Since then, there are venomous Serpents in which the fore-mentioned characters, *viz.* a broad and depreſſed head, covered with ſmall ſcales, are not to be found; I ſhall next examine whether thoſe characters are to be found in any of thoſe Serpents which are not venomous. In the genus Coluber there are very few (except venomous ones) which have the head much broader than the neck; and of thoſe few, I believe, every one has the head covered with large ſcales. But in the genus Boa, though no ſpecies is venomous, except the contortrix, almoſt every one has the head broad, depreſſed, and covered with ſmall ſcales. The canina, Conſtrictor, hortulana, beſides ſome others not deſcribed by LINNÆUS, furniſh

* The Ceraſtes is not marked by LINNÆUS as a venomous ſpecies. He probably depended upon HASSELQUIST's deſcription, which I ſuſpect to have been made from a mutilated ſpecimen. Mr. ELLIS's deſcription in the Philoſophical Tranſactions, Vol. LVI. p. 287. is only a Tranſlation of HASSELQUIST's. But he obſerves, that Dr. TURNBULL told him it was venomous. That it is ſo, I have not the ſmalleſt doubt, though in the only ſpecimen I have ſeen of it the fangs were wanting. IMPERATO, who has given a figure of it (Hiſt. Nat. p. 784. Ed. Nap.), ſays it is very venomous.

examples of this. It must, however, be confessed, that the general character of the head of the Boa, though differing very widely from that of those Colubri which are not venomous, is not quite that of the Crotalus; but the difference, though very obvious to a person accustomed to the examination of Serpents, is perhaps not easy to be fully expressed in words. It seems, however, to consist principally in a lateral compression, and elongation, of the anterior part of the head, so as to form a kind of snout. Hence the trivial name of *canina* is given by LINNÆUS to one of the species.

From the characters of the head (as the trunk affords none deserving consideration) I shall proceed to those of the other extremity.

In the Crotali I have never found the tail (exclusive of the Rattle) to exceed one-ninth part of the whole length; sometimes I have found it much shorter. In some of the venomous Colubri, the proportion is still less. In the Atropos I found it only one-thirteenth. In the English Viper (Coluber Berus) it is commonly about one-seventh or eighth. In some venomous species, however, the proportion is something greater. In the Naja I have found it as much as one-sixth; which proportion is, I believe, as great as I have ever observed: but that I may be sure to keep within the truth, I will only say, that I have never met with a venomous Serpent, the tail of which was equal to one-fifth of the whole length *.

* The tail of the Boa contortrix is said by LINNÆUS to be one-third; but his own enumeration of the Scuta sufficiently shews that this must be an error. The Coluber Leberis, Dipsas, and mycterizans appear, by the number of scales under their tail, to furnish exceptions to what I have said. The two first I have never seen, but suspect they are not venomous; that the last is not so I am very certain, having examined many specimens of it.

With reſpect to thoſe Colubri which are not venomous, it muſt be confeſſed, that there are many whoſe tails are within the limits aſſigned to the venomous ones. In the Coluber Æſculapii, doliatus, getulus, and ſome others, the tail is not, in general, more than one-ſeventh of the whole length. In the lemniſcatus I have found it not exceeding one-twelfth or thirteenth; but I know no other Linnæan ſpecies in which it is ſo ſhort. In the greater number, however, the proportion of tail is more conſiderable; in many, it is full one-third. In the Ahætulla, and in ſome ſpecies not deſcribed by LINNÆUS, I have ſeen it more than two-fifths; but have never met with a ſpecies in which it was quite ſo long as the trunk, or half of the whole length.

I have not conſidered the Boæ, becauſe none of the Linnæan ſpecies, of that genus, have their tails either remarkably long, or ſhort; but, in two ſpecies, not deſcribed by LINNÆUS, I found the tail very little exceeding the proportion I have aſſigned to the Coluber lemniſcatus.

In the thickneſs of the tail, or in the acuteneſs of its termination, I have obſerved no difference worth remarking. In every ſpecies of the three firſt genera, the tail is thinner than the trunk; and in moſt of them it is more or leſs acute. The few exceptions I have obſerved were, I believe, none of them venomous; but they are too few to deſerve any particular conſideration.

A character of great uſe in diſtinguiſhing the ſpecies of Serpents, and which was not overlooked by LINNÆUS, is, that elevated line, or carina, with which the ſcales of many ſpecies are furniſhed. In order to ſhew how far this is to be conſidered as ſerving to diſtinguiſh venomous Serpents from others, I need only obſerve, that I have examined one hundred and twelve ſpecies of Serpents, not venomous, belonging to the three firſt

genera; and find that eighty of them have ſmooth ſcales, and thirty-two only have carinated ones. Of venomous Serpents I have examined twenty-ſix; of which number, twenty have carinated ſcales, and only ſix have ſmooth ones. Upon the whole, therefore, carinated ſcales muſt be conſidered as being, in ſome meaſure, a character of venomous Serpents.

In what I have hitherto ſaid, I have conſidered only the three firſt genera of Serpents; I ſhall now make ſome remarks upon the three laſt.

Theſe three (*viz.* Anguis, Amphiſbæna, and Cæcilia), beſides the characters aſſigned them by LINNÆUS, have ſome others which are common to all, and which render them very different, in their external appearance, from any of the three firſt genera. Theſe are, a very thick and obtuſe tail, and a head which is very indiſtinct *, and furniſhed with very ſmall eyes. This laſt character (*viz.* very ſmall eyes) is ſometimes, though very rarely, met with among the Colubri, for inſtance, in the lemniſcatus; in the three laſt genera, however, it takes place, I believe, without exception. The thickneſs of the tail is alſo common to every ſpecies; and though in the Anguis bipes, and in another ſpecies, not deſcribed by LINNÆUS, but figured in BROWNE's Hiſtory of Jamaica (Tab. XLIV. fig. 1. †), the tail has an acute termination, yet in both thoſe ſpecies, eſpecially in the laſt, it continues thick to the end, and becomes ſuddenly ſharp, being what in botanical language would be called, *obtuſa cum acumine*. With reſpect to the proportionate length of tail, however, it is very remarkable, that the genus

* This indiſtinctneſs of the head, which is more or leſs common to each genus, is in the Amphiſbæna ſo conſiderable, as to have given riſe to the ſuppoſition of that Serpent's having a head at each end.

† This figure is, by LINNÆUS, erroneouſly quoted as his Anguis lumbricalis.

Anguis

Anguis affords examples of much leſs proportion, and alſo of much greater, than is to be found in any of the three firſt genera. In the Anguis Scytale the tail is not above one-twentieth of the whole length; in the maculata it is not above one-fortieth; yet in the Anguis fragilis, and in the ventralis, the tail is always longer than the trunk, or, in other words, is more than half the whole length. Indeed, in one ſpecimen of the laſt mentioned ſpecies, I found the tail nearly two-thirds of the whole length. It may, however, be queſtioned whether that ſpecies is really an Anguis, or a Lacerta *.

I ſhall make no further remarks on the external characters of Serpents; the principal inferences to be deduced from thoſe I have already made, are the following.

1ſt, That a broad head, covered with ſmall ſcales, though it be not a certain criterion of venomous Serpents, is, with ſome few exceptions, a general character of them.

2dly, That a tail under one-fifth of the whole length, is alſo a general character of venomous Serpents; but, ſince many of thoſe which are not venomous have tails as ſhort, little dependance can be placed upon that circumſtance alone. On the other hand, a tail exceeding that proportion, is a pretty certain mark that the ſpecies, to which it belongs, is not venomous.

3dly, That a thin and acute tail is by no means to be conſidered as peculiar to venomous Serpents; though a thick and obtuſe one is only to be found among thoſe which are not venomous.

* The Anguis ventralis of LINNÆUS, is ſo very like the Lacerta apoda, deſcribed by PALLAS, in Vol. XIX. of the Novi Comment. Petrop. as to render it doubtful whether it may not be the ſame. When I firſt examined it, I conſidered it as a Lacerta, on account of the projecting future along the body, and the open ears; but I have ſince met with a ſpecimen which had two large echinated *Penes* (as they are called) a character which is, I believe, peculiar to Serpents.

4thly,

4thly, That carinated ſcales are, in ſome meaſure, characteriſtic of venomous Serpents, ſince in them they are more common than ſmooth ones, in the proportion of nearly 4 to 1; whereas, ſmooth ſcales are, in thoſe Serpents which are not venomous, more common, in the proportion of nearly 3 to 1.

Upon the whole therefore it appears, that though a pretty certain conjecture may, in many inſtances, be made, from the external characters; yet, in order to determine, with certainty, whether a Serpent be venomous or not, it becomes neceſſary to have recourſe to ſome more certain diagnoſtic. This can only be ſought for in the mouth; I ſhall therefore next conſider, how the fangs, with which the mouths of venomous Serpents are furniſhed, are to be diſtinguiſhed from common teeth.

To thoſe who form their ideas of the fangs of venomous Serpents, from thoſe of the Rattle-ſnake, or even from thoſe of the Engliſh Viper, it will appear ſtrange, that there ſhould be any difficulty in diſtinguiſhing thoſe weapons from common teeth; and indeed the diſtinction would really be very eaſy, were all venomous Serpents furniſhed with fangs as large as thoſe of the fore-mentioned ſpecies. But the fact is, that in many ſpecies the fangs are full as ſmall as common teeth, and conſequently cannot, by their ſize, be known from them; this is the caſe with the Coluber laticaudatus*, lacteus, and ſeveral others. I cannot, however, better demonſtrate that the diſtinction, between the venomous fangs and common teeth, is not very obvious, than by ſhewing how very vague and erroneous

* This ſpecies is by LINNÆUS reckoned venomous, in the Muſeum Regis, though the mark is not affixed to it in the Syſtema Naturæ. To me it appears to be certainly venomous, and is the only water Serpent I have met with that is ſo.

LIN-

LINNÆUS's ideas about them were; nor can I better prove the want of information on this ſubject, than by obſerving that, erroneous as the ideas of LINNÆUS were, no one, that I know of, has yet attempted to furniſh more correct ones.

LINNÆUS thought the fangs might be diſtinguiſhed by their mobility; this, at leaſt, may be fairly inferred, from his never mentioning them in the Muſeum Regis, without adding the epithet *mobilia*, except in one inſtance (the Coluber aulicus); and, in that very inſtance, the want of mobility in the ſuppoſed fangs appears evidently to raiſe doubts in his mind, whether they are really fangs or not. His words are, "*Dentes, ſive tela, duo, rigida, parva, non mobilia.*" Theſe doubts, reſpecting the above-mentioned ſpecies, I am not able to remove, as I am not ſure that I have ever ſeen it *. But with regard to mobility, conſidered in general as a character of venomous fangs, I muſt aſſert, not only that I have never found it ſo, but alſo, that I have never been able to diſcover in them any thing which I thought could properly be called mobility. I have, indeed, ſometimes found ſome of them looſe in their ſockets; but then I have found others, in the ſame ſpecimen, quite fixed. The ſame thing was obſerved both by Dr. NICHOLLS †, and by the Abbé FONTANA ‡, in the common Viper, even during life. The looſe fangs may be ſuch as have not yet been firmly fixed in their ſocket, or they may have been looſened by ſome accident: for I ſuſpect that the fangs may be at any time looſened, and even diſplaced, by a ſmall degree of violence; and that, perhaps, may be one

* I have ſeen one, which agreed pretty well with LINNÆUS's deſcription; if that was really his ſpecies, it is not venomous.

† Appendix to Dr. MEAD's Account of the Viper.

‡ FONTANA, Traité ſur le Venin de la Vipere, chap. 1ſt and 2d.

reaſon

reason why there is always a certain number of small fangs, near the base of the full grown ones, ready to enlarge and take their place, if they should be, by any accident, torn out.

LINNÆUS seems also to have thought that the fangs might be known by their situation. In the Introduction to the class Amphibia in the Systema Naturæ, he says they are, "*Dentibus* "*similima sed extra maxillam superiorem collocata*;" and in the description of the Crotalus Dryinas, in the Amœnitates Academicæ, he says, "*Dentes ejus duo canini uti in reliquis vene-*" *natis Serpentibus non in maxillis hærent, iis enim vulnerando,* "*non autem ictus infligendo utitur.*"

These two quotations shew, that LINNÆUS thought the situation of the fangs different from that of the common teeth; the last also shews that he thought their mode of action influenced by it. What difference in situation may be found by accurate dissection, it is foreign from my present purpose to enquire; I am, however, very certain that common examination * will not discover any difference, in that respect, between the fangs of venomous Serpents, and the teeth of others.

But the most singular opinion of LINNÆUS, respecting the venomous fangs, was, that they were sometimes fixed in the base of the jaw. Of this he has given two instances in the Museum Regis. One in the description of the Coluber severus, of which he says, "*Hastæ mobiles solitariæ versus basin maxil-*" *larum interius adhærent.*" The other in that of the Coluber stolatus. His words there are, "*Tela mobilia ad basin maxil-*

* By common examination I mean such as may be made without dissecting, or otherwise damaging, the specimen to be examined; and such only do I suppose allowable in the distinction I am seeking to establish.

" *larum affixa, ut vix vulnerare valeat hoſtes, ſolum cibos veneno* " *inficere.*"

LINNÆUS's opinion reſpecting the uſe of the fangs, in the laſt mentioned ſpecies, appears to me not very clearly expreſſed *. But I have quoted both deſcriptions, merely to ſhew that LINNÆUS thought the fangs were ſometimes placed in the baſe of the jaw; an idea for which I have never been able to diſcover any foundation. The firſt of the two ſpecies in queſtion I have never ſeen; of the ſtolatus I have examined ſeveral ſpecimens, and am convinced it is not venomous.

I ſhall not dwell any longer on the falſe notions which have been entertained, reſpecting the fangs of venomous Serpents, but ſhall proceed to ſhew how, in my opinion, they may be moſt eaſily, and moſt certainly, diſtinguiſhed from common teeth.

With reſpect to their ſize, I have already obſerved that it is very various, conſequently no certain judgement can, in all caſes, be made from that circumſtance. In ſome ſpecies they are ſo large, that their ſize alone ſufficiently diſtinguiſhes them from common teeth; but in others they are ſo ſmall, that it is very difficult to diſcover them.

The ſize of the common teeth alſo varies very much, in different ſpecies. In the Coluber myſterizans they are remarkably large, eſpecially thoſe which are ſituated near the apex of the upper jaw; which circumſtance probably helped to lead LINNÆUS into the erroneous opinion he entertained, that this Ser-

* LINNÆUS's opinion ſeems not unlike that of the Abbé FONTANA, who (in the work already mentioned, chap. 12.) ſuppoſes the poiſon of the Viper may be of uſe, to the animal, in digeſtion. To me the venomous fangs have ever appeared to be merely offenſive weapons; nor can I ſee greater difficulty in ſuppoſing ſuch a weapon, with the power of injecting poiſon, placed in the head of a Viper or Rattle-ſnake, than in ſuppoſing ſuch an one, with a ſimilar power, placed in the tail of a Waſp or Hornet.

pent was venomous. But in many ſpecies the teeth are ſo ſmall, that it is impoſſible to diſcover, merely by looking into the mouth, that the animal has any. Yet in that caſe they may be very eaſily detected, by drawing a pin (or any other hard ſubſtance) with a moderate degree of preſſure, along the edge of the jaw, from the apex to the angle of the mouth, when they will be felt to grate againſt the pin, like the teeth of a ſaw.

Although the ſize of the venomous fangs is very various, their ſituation is, I believe, always the ſame; namely, in the anterior and exterior part of the upper jaw, which ſituation I conſider as the only one in which venomous fangs are ever found. But as, in thoſe Serpents which are not venomous, common teeth are found in that part of the jaw, it is plain that we cannot, by ſituation alone, diſtinguiſh one from the other. They may, however, be diſtinguiſhed with great eaſe, and I believe alſo with great certainty, by the following ſimple operation. When it is diſcovered that there is ſomething like teeth in the fore-mentioned part of the upper jaw, let a pin be drawn, in the manner already deſcribed, from that part of the jaw to the angle of the mouth (which operation may, for greater certainty, be tried on each ſide). If no more teeth are felt in that line, it may I believe be certainly concluded, that thoſe firſt diſcovered are what I have diſtinguiſhed by the name of fangs, and conſequently, that the Serpent is a venomous one *. If, on the contrary, the teeth firſt diſcovered are found not to ſtand alone, but to be only a part of a complete row, it may as certainly be concluded, that the Serpent is not venomous.

* If a ſpecimen ſhould be met with, in which no teeth, of any kind, can be diſcovered in the margin of the upper jaw, the preſumption is, that it is a venomous Serpent, which has loſt its fangs; but I have never met with ſuch an one, except the Coluber Ceraſtes already mentioned.

In the upper jaw, both of venomous Serpents and others, beſides the teeth already ſpoken of, there are two interior rows; conſequently, the diſtinction I have endeavoured to eſtabliſh might be expreſſed in other words, by ſaying, that all venomous Serpents have only two rows of teeth, in the upper jaw, and all others have four*. I think it better, however, to leave the interior rows out of the queſtion, as, in many ſpecies, the teeth of which they are compoſed are ſo ſmall, as to make it very difficult to diſcover them. Indeed, in two ſpecies of Anguis, I can hardly be ſure that I have diſcovered them; but as, in every other ſpecies, I have never failed to do ſo, I preſume I may, with very little riſk of error, aſſert, that all Serpents whatever are furniſhed with them; and that thoſe only, which are not venomous, have the exterior rows.

What I have ſaid ſufficiently ſhews that LINNÆUS's ideas, reſpecting venomous ſerpents, were ſuch as did not permit him to ſeparate them from the others; if the method I have propoſed ſhall be found to render the diſtinction of them ſufficiently clear and eaſy, it naturally follows, that they ſhould be made generically diſtinct. Some other reforms might alſo be made in LINNÆUS's claſs of Amphibia, the conſideration of which I do not mean, at preſent, to enter further into. But, before I conclude, I think it neceſſary to notice an inaccuracy of LINNÆUS, of a different kind from thoſe I have already pointed out.

* GRONOVIUS, of whoſe inaccuracy I have already given one inſtance, in deſcribing the Crotalus Duriſſus, in his Muſeum Ichthyologicum, ſays it has no teeth, except the venomous fangs. KLEIN, in his Tentamen Herpetologiæ, has gone ſtill further, having actually made a genus of Serpents without teeth, which he calls Anodon. He appears not to have examined the mouth of a ſingle ſpecies; but to have depended intirely upon the deſcriptions of SEBA.

In the Preface to the Muſeum Regis, and in the Introduction to the claſs Amphibia, in the Syſtema Naturæ, LINNÆUS ſays, that the proportion of venomous Serpents to others, is 1 in 10; yet in the Syſtema Naturæ, in which the ſum total of ſpecies is one hundred and thirty-one, he has marked twenty-three as venomous, which is ſomewhat more than 1 in 6. How he came to be ſo much at variance with himſelf, I know not; but the laſt mentioned proportion ſeems to me to be not far from the truth; as I find that I have examined one hundred and fifty-four ſpecies of Serpents, of which number twenty-ſix appear to be venemous.

I have already mentioned, that the Coluber ſtolatus and the mycterizans, though marked by LINNÆUS as venomous Serpents, certainly are not ſo; and that I ſuſpect the ſame may be ſaid of the Leberis, and Dipſas. I have alſo obſerved, that the Boa contortrix, Coluber Ceraſtes, and laticaudatus, none of which are marked in the Syſtema Naturæ, are all of them venomous; to theſe laſt may be added the Coluber fulvus.

If LINNÆUS's ſpecies were all accurately examined, I have no doubt but more errors, of both kinds, would be found; for it muſt be obſerved, that though I have examined a greater number of ſpecies than LINNÆUS, not above half that number are of thoſe deſcribed by him; conſequently there remains more than one-third of his ſpecies which I have never ſeen. The number I have examined, however, ſeems to me ſufficiently great to warrant the inferences I have drawn from that examination. That ſome exceptions to them might be found, by the examination of a greater number, is very poſſible; but, if theſe obſervations ſhall tend to rectify the falſe notions which have been entertained reſpecting venomous Serpents, and to render the diſtinction between them and others more clear, I truſt they will be thought not totally uſeleſs.

IV. *Observations on the Dryneſs of the Year* 1788. *In a Letter from the Rev. Mr.* B. Hutchinſon *to Sir* Joſeph Banks, *Bart. P. R. S.*

Read January 15, 1789.

SIR, Kimbolton, January 8, 1789.

AS the defect of rain has been very conſiderable in 1788; and in conſequence a great want of water on the cloſe of the year univerſally felt; perhaps the quantity fallen here, compared with that of the ſeven preceding years, may not be unacceptable to yourſelf and the Royal Society.

	Inches.	
Rain 1781	21,6	
1782	32,3	
1783	23,6	
1784	28,0	$\frac{175}{5} = 25$ inches, the mean of ſeven years.
1785	21,0	
1786	24,7	
1787	23,8	
1788	14,5	

By eſtimation it therefore appears, that the average quantity of rain of the ſeven preceding years is 25 inches, and the rain which fell laſt year is only 14,5, that is, not much more than half that quantity, if we deduct 1,3 now lying in ſnow, which fell in December, and not in ſolution. On the ſuppoſition

ſition which, I believe, is not far from truth, that the whole iſland has had the ſame defect; a greater failure of the produce of the earth might have been expected than what the country has experienced; for, except in hay, and a little failure in turneps, the crops have in general been as plentiful as in moſt of the former years, and in fruits of the orchard much more ſo.

It has always been ſaid of England, that drought never occaſions want; this year verifies the aſſertion. But to account for crops that, taken on the whole, are rather abundant, we may conſult the following monthly ſtate of rain for 1788.

	Inches.
In January	0,3
February	1,7
March	0,7
April	0,0
May	0,6
June	1,8
July	0,8
Auguſt	3,4
September	3,4
October	0,3
November	0,2
December	1,3
	14,5

Having premiſed, Sir, that there were no extremes of cold and heat throughout the year; the thermometer in a northern expoſure never falling below the freezing point during the day-time, except on the 14th and 15th of January, the 6th, 7th,

7th, 8th, 10th, 11th, 12th, 13th, and 17th of March, and on none of thoſe days at noon, ſo that there never were twenty-four hours together ſucceſſive froſt; therefore vegetation was never entirely at a ſtand. In ſummer it did not riſe to 80 degrees, except on

		Deg.	
May	26	80	
	27	81	
	28	81	
June	18	83	with thunder and rain: then cool for a
	27	80	week.
July	11	80	
	12	82	
Auguſt	4	81	: the reſt of the time exceedingly temperate.

Now, the rain that fell on February was towards the end of the month; which, together with that which fell in March, brought up the ſpring corn, gave an early firſt crop of hay to the large towns, and covered the meadows and paſtures in the country; that they were not ſo entirely dried up through the defect of April, as to prevent the rain, which fell plentifully on the 29th of May, ſucceeded by more in June, giving a ſecond crop to the former ſituations, and a firſt, though late one, to the latter: and as fructification chiefly depends on rain falling at the latter end of the ſeaſon of flowering, this rain ſet the bloſſoms of wheat, and of the uſeful fruit-trees; as the great rains in Auguſt ſwelled the kernel, filled, as they term it, the buſhel, and gave an opportunity for a ſecond crop of turneps that proved more vigorous than the firſt.

V. *On the Method of determining, from the real Probabilities of Life, the Value of a contingent Reverſion in which Three Lives are involved in the Survivorſhip. By Mr.* William Morgan; *communicated by the Rev.* Richard Price, *D. D. F. R. S.*

Read January 29, 1789.

IN a Paper which I had lately the honour of communicating to the Royal Society, reſpecting the method of determining the values of reverſions depending on ſurvivorſhips between two perſons from the real probabilities of life, I obſerved, that the inveſtigation of thoſe caſes in which three lives were involved in the ſurvivorſhip (though attended with much more difficulty) might, however, be effected in a ſimilar manner. The further purſuit of this ſubject has now convinced me that, as it is never ſafe, ſo likewiſe it can never be neceſſary to have recourſe to the *expectations* of life in any caſe; and that the ſolution even of thoſe problems which include three lives is far from being ſo formidable as at firſt ſight it appears to be. I am ſenſible of the impropriety of entering minutely in this place into the vaſt variety of propoſitions which refer to the different orders of ſurvivorſhip between three lives; but as the following problem ſeems to be of conſiderable importance on account of its being applied to the ſolution of many other problems, the demonſtration of it, perhaps, may not be thought an improper addition to my former Paper.

PROBLEM.

PROBLEM.

Supposing the ages of A, B, and C, to be given; to determine, from any table of observations, the value of the sum S payable on the contingency of C's surviving B, provided the life of A shall be then extinct.

SOLUTION.

Let a represent the number of persons living in the table at the age of A. Let a', a'', a''', a'''', &c. represent the decrements of life at the end of the 1st, 2d, 3d, 4th, &c. years from the age of A. Let b represent the number of persons living at the age of B, and m, n, o, p, &c. the number of persons living at the end of the 1st, 2d, 3d, 4th, &c. years from the age of B. In like manner let c represent the number of persons living at the age of C, and d, e, f, g, &c. the number of persons living at the end of the 1st, 2d, 3d, 4th, &c. years from the age of C. Let r also denote the value of £. 1 increased by its interest for a year. In order to receive the sum S in the first year, it is necessary either that all the three lives shall have died in that year, A having died first, B next, and C last; or that only the two lives A and B shall have died (A having died first), and that C shall have lived to the end of that year. The probability that the three lives shall die in the first year is $\frac{a' \cdot \overline{b-m} \cdot \overline{c-d}}{abc}$. The probability that they shall die in the order above mentioned is $\frac{a' \cdot \overline{b-m} \cdot \overline{c-d}}{b \cdot abc}$. The probability that both A and B shall die in the first year is $\frac{a' \cdot \overline{b-m}}{ab}$. Half this fraction,

tion, or $\frac{a' \cdot \overline{b-m}}{2ab}$, is the probability that the death of A ſhall happen before the death of B in this year. The probability that C ſhall ſurvive A and B, reſtrained to the contingency of A's having died firſt, is $\frac{a' \cdot \overline{b-m} \cdot d}{2abc}$. The value therefore of the ſum S for the firſt year is $S \times \frac{a' \cdot \overline{b-m} \cdot \overline{c-d}}{6 \cdot abcr} + \frac{a' \cdot \overline{b-m} \cdot d}{2abcr} = \frac{S}{abcr} \times \frac{a'bc}{6} - \frac{a'mc}{6} + \frac{a'db}{3} - \frac{a'md}{3}$ ..... In the ſecond year the payment of the given ſum will depend on either of four events happening. Firſt, on the contingency of all the three lives dying in that year, A having died firſt, B next, and C laſt. 2dly, On the contingency of B's dying in that year, C's living to the end of it, and A's dying in the firſt year. 3dly, On the contingency of B's dying after A in the ſecond year (both of them having ſurvived the firſt year) and of C's living to the end of that year. 4thly, On the contingency of A's dying in the firſt year, and of B and C's both dying in the ſecond year, B having died firſt. The probability of the firſt contingency is expreſſed by the fraction $\frac{a'' \cdot \overline{m-n} \cdot \overline{d-e}}{6 \cdot abc}$. The probability of the ſecond by the fraction $\frac{a' \cdot \overline{m-n} \cdot e}{abc}$. The probability of the third by the fraction $\frac{a'' \cdot \overline{m-n} \cdot e}{2 \cdot abc}$. And the probability of the fourth contingency by the fraction $\frac{a' \cdot \overline{m-n} \cdot \overline{d-e}}{2abc}$. Theſe ſeveral fractions, therefore, multiplied into $\frac{S}{r}$ will be the value of the given ſum for the ſecond year, and may be eaſily found $= \frac{S}{abcr^2} \times \frac{a''dm}{6} - \frac{a''dn}{6} + \frac{a''en}{3} - \frac{a''em}{3} + \frac{a'em}{2} - \frac{a'en}{2} + \frac{a'dm}{2} - \frac{a'dn}{2}$. In like

manner the payment of the given ſum in the third year will depend on the contingency of the ſame number of events as in the ſecond year; that is, it will, firſt, depend on the contingency of all the three lives dying in that year, A having died firſt, B next, and C laſt; 2dly, on the contingency of B's dying in that particular year, C's living to the end of it, and A's dying in the firſt or ſecond years; 3dly, on the contingency of B's dying after A in the third year (both of them having ſurvived the two preceding years), and of C's living to the end of that year; and, 4thly, on the contingency of A's dying in the firſt or ſecond year, and of B and C's both dying in the third year, C having died laſt. Theſe ſeveral contingencies are expreſſed by the reſpective fractions $\frac{a''' \cdot \overline{n-o} \cdot \overline{e-f}}{6 \cdot abc}$ ... $\frac{\overline{a'+a''} \cdot \overline{n-o} \cdot f}{6bc}$ ... $\frac{a'' \cdot \overline{n-o} \cdot f}{2abc}$ ... and $\frac{\overline{a'+a''} \cdot \overline{n-o} \cdot \overline{e-f}}{2abc}$. Conſequently the value of the ſum S for the third year will be $= \frac{S}{abcr^3} \times \frac{a''' \cdot en}{6} - \frac{a''' \cdot eo}{6} + \frac{a''' \cdot fo}{3} - \frac{a''' \cdot fo}{3} + \frac{\overline{a'+a''} \cdot fn}{2} - \frac{\overline{a'+a''} \cdot fo}{2} + \frac{\overline{a'+a''} \cdot en}{2} - \frac{\overline{a'+a''} \cdot eo}{2}$. And by reaſoning in the ſame manner the value of the ſum S for the fourth year may be found $= \frac{S}{abcr^4} + \frac{a'''' \cdot fo}{6} - \frac{a'''' \cdot fp}{6} + \frac{a''' \cdot go}{3} - \frac{a'''' \cdot gp}{3} + \frac{\overline{a'+a''+a'''} \cdot go}{2} - \frac{\overline{a'+a''+a'''} \cdot gp}{2} + \frac{\overline{a'+a''+a'''} \cdot of}{2} - \frac{\overline{a'+a''+a'''} \cdot fp}{2}$.

If either B or C be the oldeſt of the three lives, theſe ſeries continued to the extremity of that life will expreſs the whole value of the reverſion, which will be $= \frac{S}{b} \times \frac{a'bc}{abcr} + \frac{a''dm}{abcr^2} + \frac{a'''en}{abcr^3} + \frac{a''''fo}{abcr^4} + \&c. + \frac{S}{2r} \times \frac{a'dm}{abcr} + \frac{a'+a'' \cdot en}{abcr^2} +$

$\overline{\frac{a'+a''+a'''\cdot fo}{abcr^3}}$ + &c. $-\frac{S}{6}\times\overline{\frac{a'mc}{abcr}+\frac{a''na}{abcr^2}+\frac{a'''oe}{abcr^3}+\frac{a''''\cdot pf}{abcr^4}}$ + &c. $-$

$\frac{S}{2r}\times\overline{\frac{a'dn}{abcr}+\frac{a'+a''\cdot eo}{abcr^2}+\frac{a'+a''+a'''\cdot fp}{abcr^3}}$ + &c. $+\frac{S}{3}\times\overline{\frac{a'ab}{abcr}+\frac{a''\cdot en}{abcr^2}}+$

$\overline{\frac{a'''\cdot fn}{abcr^3}+\frac{a''''\cdot go}{abcr^4}}$ + &c. $+\frac{S}{2r}\times\overline{\frac{a'em}{abcr}+\frac{a'+a''\cdot fn}{abcr^2}+\frac{a'+a''+a'''\cdot go}{abcr^3}}$ + &c.

$-\frac{S}{3}\times\overline{\frac{a'md}{abcr}+\frac{a''en}{abcr^2}+\frac{a'''\cdot fo}{abcr^3}+\frac{a''''\cdot gp}{abcr^4}}$ + &c. $-\frac{S}{2r}\times\overline{\frac{a'en}{abcr}+\frac{a'+a'\cdot fo}{abcr^2}}+$

$\overline{\frac{a'+a''+a'''\cdot gp}{abcr^3}}$ + &c.

In order to ſum up the firſt and ſecond of theſe ſeries let β repreſent the number of perſons living at the age of F, a perſon one year younger than B, and $\varkappa$ the number of perſons living at the age of K, a perſon one year younger than C. Let FK, BC, AFK, and ABC, repreſent the value of an annuity on the two and three joint lives of F and K, of B and C, of A, F and K, and of A, B and C reſpectively; then will the ſeries $\frac{S}{6}\times\overline{\frac{a'bc}{abcr}+\frac{a''dm}{abcr^2}+\frac{a'''en}{abcr^3}}$ + &c. be $=\frac{S\cdot\beta\cdot\varkappa}{6\cdot bc}\times$

$\frac{bc}{\beta\varkappa r}-\overline{\frac{a-a'\cdot bc}{a\beta\varkappa r}+\frac{dm}{\beta\varkappa r^2}}-\overline{\frac{a-a'-a''\cdot dm}{a\beta\varkappa r^2}+\frac{a'dm}{a\beta\varkappa r^2}+\frac{en}{\beta\varkappa r^3}-\frac{a-a'-a''-a'''\cdot en}{a\beta\varkappa r^3}}$

$-\overline{\frac{a'+a''\cdot en}{a\beta\varkappa r^3}}$, &c. $=\frac{S\cdot\beta\varkappa}{bc}\times\frac{FK-AFK}{6}\Big(-\frac{S}{6r}\times\overline{\frac{a'dm}{abcr}+\frac{a'+a''\cdot en}{abcr^2}}$, &c.

$=-\frac{S}{6r}\times\overline{\frac{dm}{bcr}-\frac{a-a'\cdot dm}{abcr}+\frac{en}{bcr^2}-\frac{a-a'-a''\cdot en}{abcr^2}}$, &c. $=\Big)-\frac{S}{6r}\times$

$\overline{BC-ABC}$. The ſum, therefore, of the two firſt ſeries, or of $\frac{S}{6}\times\overline{\frac{a'bc}{abcr}+\frac{a''\cdot dm}{abcr^2}}$ + &c. $+\frac{S}{2r}\times\overline{\frac{a'dm}{abcr}+\frac{a'+a''\cdot en}{abcr^2}}$ + &c. is $=\frac{S}{6}\times$

$\frac{\beta\varkappa\cdot\overline{FK-AFK}}{bc}+\frac{S}{3r}\times\overline{BC-ABC}$. Again, let P repreſent a life one year older than B, and let BK, PC, ABK, and APC, repreſent the values of annuities on the two and three joint lives of B and

B and K, P and C, A, B and K and of A, P and C: then the ſum of the third and fourth ſeries, or of $-\frac{S}{6}\times\overline{\frac{a'mc}{abcr}+\frac{a''nd}{abcr^2}}$ &c. $-\frac{S}{2r}\times\overline{\frac{a'dn}{abcr}+\frac{a'+a''.eo}{abcr^2}}$ &c. being $=-\frac{S.x}{6c}\times\overline{\frac{mc}{xbr}-\frac{a-a'.mc}{abxr}+\frac{dn}{bxr^2}-\frac{a-a'-a''.dn}{abxr^2}+\frac{eo}{bxr^3}-\frac{a-a'-a''-a'''.eo}{abxr^3}}$, &c. $\left(-\frac{S}{3r}\times\overline{\frac{a'dn}{abcr}+\frac{a'+a''.eo}{abcr^2}}\right.$, &c. $=)-\frac{S.m}{3br}\times\overline{\frac{a'dn}{acmr}+\frac{a''+a''.eo}{acmr^2}}$ &c., will be $=-\frac{S}{6}\times\frac{x.\overline{BK-ABK}}{c}-\frac{S}{3r}\times\frac{m.\overline{FC-APC}}{b}$. The fifth ſeries, $\frac{S}{3}\times\overline{\frac{a'db}{abcr}+\frac{a''em}{abcr^2}}$ &c., is $=\frac{S}{3}\times\frac{\beta}{b}\times\overline{\frac{db}{\beta cr}-\frac{a-a'.db}{a\beta cr}+\frac{em}{\beta cr^2}-\frac{a-a'-a''.em}{a\beta cr^2}+\frac{fn}{\beta cr^3}-\frac{a-a'-a''-a'''.fn}{a\beta cr^3}}$, &c. $-\frac{S}{3r}\times\overline{\frac{a'em}{abcr}+\frac{a'+a''.fn}{abcr^2}}$, &c. $=$ (putting FC and AFC for the values of the two and three joint lives of F and C and of A, F, and C) $\frac{S}{3}\times\frac{\beta.\overline{FC-AFC}}{b}-\frac{S}{3r}\times\overline{\frac{a'em}{abcr}+\frac{a'+a''.fn}{abcr^2}}$, &c. The ſum, therefore, of the fifth and ſixth ſeries, or of $\frac{S}{3}\times\overline{\frac{a'db}{abcr}+\frac{a''em}{abcr^2}}$, &c. $+\frac{S}{2r}\times\overline{\frac{a'em}{abcr}+\frac{a'+a''.fn}{abcr^2}}$ &c. is $=\frac{S}{3}\times\frac{\beta.\overline{FC-AFC}}{b}+\left(\frac{S}{6r}\times\overline{\frac{a'em}{abcr}+\frac{a'+a''.fn}{abrc^2}}\right.$, &c. $=)\frac{S\times d}{6cr}\times\overline{\frac{em}{bdr}-\frac{a-a'.em}{abdr}+\frac{fn}{bdr^2}-\frac{a-a'-a''.fn}{abdr^2}}$ &c. If T denote a life one year older than C, and BT, and ABT denote the values of the two and three joint lives of B and T and of A, B, and T, this laſt ſeries will be $=\frac{S}{6r}\times\frac{d.\overline{BT-ABT}}{c}$, and conſequently the ſum of the fifth and ſixth ſeries will be $=\frac{S}{3}\times\frac{\beta.\overline{FC-AFC}}{b}+\frac{S}{6r}\times\frac{d.\overline{BT-ABT}}{c}$. Laſtly, the ſeventh and eighth ſeries, or $-\frac{S}{3}$

$\frac{S}{3} \times \frac{a'md}{abcr} + \frac{a''en}{abcr^2} + \&c. - \frac{S}{2r} \times \frac{d'en}{abcr} + \frac{d'+a''.fo}{abcr^2} + \&c.$ are $= -\frac{S}{3} \times \frac{dm}{bcr} - \frac{a-a'.dm}{abcr} + \frac{en}{bcr^2} - \frac{a-a'-a''.en}{abcr^2} + \&c. - \frac{S}{6r} \times \frac{a'en}{abcr} + \frac{a'+a''.fo}{abcr^2} + \&c.$

$= -\frac{S}{3} \times \overline{BC - ABC} - \left(\frac{S}{6r} \times \frac{md}{bc} \times \frac{en}{mdr} - \frac{a-a'.en}{amdr} + \frac{of}{mar^2} - \frac{a-a'-a''.of}{amdr^2} + \&c. = \right) \frac{S}{6r} \times \frac{md.PT-APT}{bc}$, where PT and APT repreſent the values of the two and three joint lives of P and T, and of A, P, and T. If theſe ſeveral expreſſions be added together, &c. we ſhall at laſt have $\frac{S.s}{6c} \times \frac{g.FK-AFK}{b} - BK - ABK + \frac{S.\beta}{3^b} \times FC - AFC - \frac{S.r-1}{3r} \times BC - ABC - \frac{S.m}{3br} \times PC - APC + \frac{S.d}{6cr} \times BT - ABT - \frac{m.PT-APT}{b}$, for the value of the ſum S, when either B or C are the oldeſt of the three lives.

In order to determine the value of the reverſion when the life of A is the oldeſt of the three lives, let s, t, u, w, &c. be the number of perſons living at the end of the 1ſt, 2d, 3d, 4th, &c. years from the age of A, and let b', b'', b''', b'''', &c. be the decrements of life at the end of 1, 2, 3, 4, &c. years from the age of B; then, by reaſoning as above, the value of the ſum S for the firſt year will be expreſſed by the ſeries $\frac{S \times b'.a-s.c-d}{6abcr} + \frac{S \times b'.a-s.d}{2abcr}$, for the ſecond year by the ſeries $\frac{S.b''.s-t.d-e}{6abcr^2} + \frac{S.b''.s-t.e}{2abcr^2} + \frac{S.b''a-s.e}{abcr^2} + \frac{S.b''.u-s.a-e}{2abcr^2}$, for the third year by the ſeries $\frac{S.b'''.t-u.e-f}{6abcr^3} + \frac{S.b'''.t-u.f}{2abcr^3} + \frac{S.b'''.a-t.f}{abcr^3} + \frac{S.b'''.a-t.e-f}{2abcr^3}$, and ſo on for the remaining years of A's life.

Theſe ſeveral ſeries may be found $= \frac{S}{abcr} \times -\frac{ab'c}{3} - \frac{b'cs}{6} - \frac{ab'd}{6} - \frac{b'ds}{3} +$

$ab'd$

$\frac{ab'd}{2}+\frac{ab'c}{2}+\frac{s}{abcr^2}\times\overline{-\frac{sb''d}{3}-\frac{b''ds}{6}-\frac{sb''e}{6}-\frac{be''s}{3}+\frac{ab''e}{2}+\frac{ae''d}{2}}+\frac{s}{abcr^3}\times\overline{-\frac{sb'''e}{3}-\frac{b'''esu}{6}-\frac{sb'''f}{6}-\frac{b'''fsu}{3}+\frac{ab'''f}{2}+\frac{ab'''e}{2}}$, &c. &c. Let a represent the number of persons living at the age of H, a person one year younger than A; let N denote a person one year older than A, and let the several combinations BN, BNC, AB, &c. denote, as in the former case, the values of annuities on the joint lives of B and N, of B, N and C, of A and B, &c.; then by proceeding in the same manner as in the foregoing demonstration the series $\frac{ab'c}{3abcr}+\frac{sb''d}{3abcr^2}+\frac{tb'''e}{3abcr^3}+$ &c. may be found $=\frac{au}{3ac}\times\overline{HK-HBK}-\frac{AC-ABC}{3r}$; the series $\frac{b'cs}{6abcr}+\frac{b''dt}{6abcr^2}+\frac{b'''su}{6abcr^3}+$ &c. $=\frac{u}{6c}\times\overline{AK-ABK}-\frac{s}{6ar}\times\overline{NC-NBC}$; the series $\frac{ab'd}{6abcr}+\frac{sb''e}{6abcr^2}+\frac{tb'''f}{6abcr^3}+$ &c. $=\frac{u}{6a}\times\overline{HC-HBC}-\frac{d}{6cr}\times\overline{AT-ABT}$; and the series $\frac{b'ds}{3abcr}+\frac{b''et}{3abcr^2}+\frac{b'''fu}{3abcr^3}+$ &c. $=\frac{AC-ABC}{3}-\frac{sd}{3acr}\times\overline{NT-NTB}$. These four series, therefore, supposing them all to be positive quantities are $=\frac{u}{3c}\times\overline{\frac{a\,.\,HK-HBK}{a}+\frac{AK-ABK}{2}}+\frac{u}{6a}\times\overline{HC-HBC}+\frac{r-1}{3r}\times\overline{AC-ABC}-\frac{s}{6ar}\times\overline{NC-NBC}-\frac{d}{3cr}\times\overline{\frac{AT-ABT}{2}+\frac{s\,.\,NT-NBT}{a}}$. With respect to the two remaining series $\frac{b'd}{2bcr}+\frac{b''e}{2bcr^2}+\frac{b'''f}{2bcr^3}+$ &c. . . . and $\frac{b'c}{2bcr}+\frac{b''d}{2bcr^2}+\frac{b'''e}{2bcr^3}+$ &c., these, it is evident, are to be continued after the decease of A till the extinction of the joint lives of B and C, and have been already proved in the solution of the second problem in my former Paper, to denote the value of the given sum payable if C should survive B. Let this value be represented

ſented by R and the ſum of the foregoing expreſſions (or $\frac{x}{3c} \times \overline{\frac{a \cdot HK - HBK}{a} + \frac{AK - ABK}{2}} + \frac{a}{6a} \times \overline{HC - HBC}$, &c.) by M, then will the value of the ſum S (when A is the oldeſt of the three lives) be $= S \times \overline{R - M}$. Q. E. D.

If the three lives be equal, the value of the given ſum for the firſt year will be $= \frac{S \cdot \overline{c-d}^3}{6 \cdot c^3 \cdot r} + \frac{S \cdot \overline{c-d}^2 \cdot d}{2c^3 \cdot r} = S \times \overline{\frac{1}{6r} + \frac{2d^3}{6c^3r} - \frac{3dd}{6c^2r}}$; the value of the ſame ſum for the ſecond year will be $= \frac{S \cdot \overline{d-e}^3}{6c^3r^2} + \frac{S \cdot \overline{d-e}^2 \cdot e}{2c^3r^2} + \frac{S \cdot \overline{d-e} \cdot \overline{c-d} \cdot e}{c^3r^2} + \frac{\overline{d-e}^2 \cdot \overline{c-d}}{2c^3r^2} = S \times \overline{\frac{2e^3}{6c^3r^2} - \frac{3ee}{6c^2r^2} - \frac{2d^3}{6c^3r^2} + \frac{3dd}{6c^2r^2}}$; the value for the third year will be $= S \times \overline{\frac{2f^3}{6c^3r^3} - \frac{3ff}{6c^2r^3} - \frac{2e^3}{6c^3r^3} + \frac{3ee}{6c^2r^3}}$, and ſo on for the other years to the extremity of life. Let CC and CCC denote the values of the two equal and three equal joint lives, the ſum of theſe ſeries may then be found $= \frac{S}{6} \times \overline{\frac{1}{r} + 2CCC - 3CC + \frac{3 \cdot CC - 2CCC}{r}}$ = (ſuppoſing the perpetuity, or $\frac{1}{r-1}$, to be denoted by V) $\frac{S}{6} \times \frac{r-1}{r} \times \overline{V - 3 \cdot CC - 2CCC}$.

It muſt be here remembered, that from other principles it is well known, that the number of years purchaſe expreſſing the value of an *eſtate* or *perpetual annuity* to be entered upon at the failure of two out of any three equal lives is, "the difference "between three times the values of two equal joint lives, and "twice the values of three equal joint lives ſubtracted from "the perpetuity," or $V - \overline{3CC - 2CCC}$. The value, therefore, of ſuch a reverſion, ſuppoſing it to depend on the failure of the three equal lives in any one particular order, is (ſince

there are ſix ſuch orders equally probable) $\frac{1}{4} \times \overline{V - 3CC - 2CCC}$. But it appears, from the correction explained in Dr. PRICE's Treatiſe on Reverſionary Payments, Vol. I. p. 34. that the value of a reverſionary *ſum* is always leſs than the value of an equivalent reverſionary *eſtate* in the proportion of 1 to r. The ſum being S the equivalent eſtate or perpetual annuity is always $S \times \overline{r-1}$; and conſequently the value of the ſum S depending on the ceaſing of three equal lives in any one particular order and thus determined, is the ſame with that determined by the foregoing inveſtigation, that is, $\frac{S}{6} \times \frac{\overline{r-1}}{r} \times \overline{V - 3CC - 2CC}$. The inveſtigation, therefore, is right, and the correction and inveſtigation demonſtrate one another.

But the foregoing expreſſion for determining the value of the reverſion in this particular caſe is not only obtained immediately from the ſeries, but alſo from the two different rules which have been given for determining the value when the lives are unequal; and hence a proof ariſes of the truth of theſe rules, as well as of the reaſoning upon which they are founded. Thus the firſt rule, ſuppoſing the lives all equal, becomes $\frac{s^3}{c^3} \times \frac{KK-CKK}{6} - \frac{dd}{cc \cdot r} \times \frac{TT-CTT}{6} - \frac{\overline{r-1}}{r} \times \frac{CC-CCC}{3} + \frac{s}{c} \times \frac{CK-CCK}{6} - \frac{d}{cr} \times \frac{CT-CCT}{6}$, and the ſecond rule becomes $\frac{\overline{V-CC} \cdot \overline{r-1}}{2r} - \frac{s^3}{c} \times \frac{KK-CKK}{3} + \frac{dd}{cc \cdot r} \times \frac{TT-CTT}{3} - \frac{r-1}{r} \times \frac{CC-CCC}{3} - \frac{s}{c} \times \frac{CK-CCK}{3} + \frac{d}{cr} \times \frac{CT-CCT}{3}$. Let the value according to the firſt rule be denoted by L, and the ſecond rule will be $= \frac{\overline{r-1} \cdot \overline{V-CC}}{2r} - 2L - \frac{\overline{r-1} \cdot \overline{CC \cdot CCC}}{r} (= L)$. Hence $3L =$

$\frac{r-1 \cdot \sqrt{CC-2 \cdot r-1 \cdot CC-CCC}}{2r}$ and $L = \frac{r-1}{6r} \times \sqrt{V-3CC-2CCC}$.

Q. E. D.

Were we poſſeſſed of complete tables of the values of annuities on two and three joint lives, the preceding rules would give an eaſy and exact ſolution of this problem in all caſes. But as ſuch tables, computed for every age, would be a work of immenſe difficulty, eſpecially in regard to the values of three joint lives, Mr. SIMPSON's rule for approximating to theſe from the given values of the two joint lives, has hitherto been adopted, and it ſeems upon the whole to anſwer the purpoſe very well. In the preſent problem it is attended with no other inconvenience than increaſing the labour of the computations; for the values of the reverſions derived from it appear in general to be perfectly correct. This is more fully aſcertained by a table which Dr. PRICE has given in his Treatiſe on Reverſionary Payments (Vol. II. Table 37.), of the values of three equal joint lives computed at 4 *per cent.* from the probabilities of life at NORTHAMPTON. By the aſſiſtance of this table, when the lives are of the ſame age, it is evident, from what has been already obſerved, that the exact value of the reverſion may be eaſily obtained. The few following ſpecimens computed from it, and compared with the values of the reverſions deduced from the firſt and ſecond of the preceding rules, demonſtrate the accuracy of thoſe rules: for, notwithſtanding the approximated values of the three joints lives have been uſed in every inſtance in which the rules have been employed, yet the reſults approach ſo near the truth, even in the laſt ſtages of life, when the decrements are moſt irregular, that, though derived from theſe approximations, there can be little doubt of their correctneſs in almoſt every other period of life.

Common age.	Exact value * of £. 100. computed from Dr. PRICE's Tables of the values of two and three equal joint lives.	Value of £. 100. computed from the first of the foregoing rules, and from Mr. SIMPSON's approximation to the values of three joint lives.	Value of £. 100. computed from the second of the foregoing rules, and from Mr. SIMPSON's approximation to the values of three joint lives.
70	12.000	12.005	12.000
75	12.944	12.943	12.943
80	13.840	13.810	13.880
85	14.450 †	14.670	14.340

Mr. DODSON ‡, and Mr. SIMPSON §, are the only writers who have solved, or rather who have approximated to the solution of this problem. But the former, by deducing his rules immediately from a wrong hypothesis, having rendered

* That is, *one-sixth* part of the whole reversion.

† The several reversions in this column, when computed from SIMPSON's approximation to the values of the three joint lives, are 12.012, 12.933, 13.847, and 14.803 respectively; which upon the whole differing nearly as much from the real values as those in the two other columns afford a convincing proof, that the very small deviation from the truth in these latter values proceeds not from any inaccuracy in the rules themselves, but solely from having used the *approximated* instead of the *real* values of the three joint lives. And this also will account for the difference in the values by the first and second rules. Were those values computed from tables which give the correct values of two and three joint lives at all ages, they would come out exactly the same. In the two first examples, where the values by one rule are true, it appears, that the values by the other rule are equally so. In the two last examples, where the values are not quite so accurate, it may be observed, that they differ as much in excess by one rule as they do in defect by the other; which must in general be the case from the very nature of those rules; for if L (or the value by the first rule) be greater than the truth, the difference between $\frac{r-1 \cdot \sqrt{-CC}}{2r}$ and 2L (or the value by the second rule) must be less than the truth; and, on the contrary, if L be less, this difference will be greater than the truth.

‡ See DODSON's Mathematical Repository, Vol. III. Questions 42, 43, &c.

§ See SIMPSON's Select Exercises, Prob. 38.

moſt of them (eſpecially thoſe in which three lives are concerned) of no uſe, it will be unneceſſary to take notice of what he has done on the ſubject. With regard to the latter, whoſe rule is not only the ſole guide for determining the value of this reverſion, but alſo the ſource from which a great variety of other problems are ſolved, perhaps it may not be improper to examine how far his ſolution is to be depended upon; and the following examples have therefore been computed for this purpoſe.

TABLE I.

Ages of C.	B.	A.	Value, by SIMPSON's rule, of £. 100. payable on the contingent reverſion ſpecified in this problem, when either C or B are eldeſt, according to the *Northampton* Table and at 4 *per cent.*	True value of the ſame reverſion computed from the firſt rule in the foregoing ſolution.
80	70	40	1.926	1.179
75	65	25	1.873	1.032
65	50	15	2.090	1.690
70	80	40	6.615	6.117
50	65	15	5.580	3.879
78	78	20	2.583	1.982
45	60	12	5.571	4.133
60	45	12	2.292	1.686

TABLE II.

Ages. C.	B.	A,	Value of the ſame reverſion by SIMPSON's rule, when A is the oldeſt of the three lives.	True value of the ſame reverſion by the ſecond rule in the foregoing ſolution.
24	65	75	34.636	31.792
65	24	75	6.305	7.895
49	9	69	7.351	5.960
18	78	78	37.554	33.019

TABLE

TABLE III.

Common Age.	Value of the ſame reverſion by SIMPSON's rule, when the ages of the three lives are equal.	True value of the ſame reverſion.
70	13.20	12.00
75	14.98	12.94
80	16.58	13.84
85	17.86	14.45

By comparing the values in the preceding tables, Mr. SIMPSON's rule appears in almoſt every inſtance to be exceedingly incorrect. Even when the lives are equal (in which caſe it might have been expected to be ſufficiently accurate) it ſeems to deviate, in old age at leaſt, ſo widely from the truth as to be unfit for uſe. When C or B are eldeſt (which, however, is a caſe that does not often occur), the reſults ſometimes exceed the truth *one-half*, and generally by more than *one-third* of the real value. When A is the oldeſt of the three lives (which is the moſt common caſe) theſe reſults are erroneous in nearly an equal degree. Nay, in ſome caſes, Mr. SIMPSON's rule is not only wrong but abſurd. Thus, in the laſt example in the ſecond table, the value of £. 100. payable on the contingency of C aged 18 ſurviving B aged 78 after A aged 78, is by this rule = £. 37.554. The value, therefore, of the ſame ſum on the contingency of C's ſurviving A after B is alſo £. 37.554. Hence the value of £. 100. on the contingency of C's ſurviving A and B (without the reſtriction of one dying before the other) is $2 \times 37.554 =$ £. 75.108 *. By another rule of Mr. SIMPSON †, the value

* See SIMPSON's Select Exerciſes, Prob. 39. † Ibid. Prob. 32.

of

of £. 100, on the contingency of C's ſurviving B only, is no more than £. 74 *. Now it is ſelf-evident, that this latter value, inſtead of being *leſs*, ought to have been *greater* than the former, inaſmuch as the probability of C's ſurviving only one life muſt be greater than that of his ſurviving two lives.

Many additional inſtances might be produced in which this rule, being made the baſis upon which the ſolutions of other problems are founded, leads to concluſions equally erroneous. But theſe enquiries would be improper here; and I ſhall only obſerve, that had the foregoing examples been computed from the SWEDEN or LONDON, inſtead of the NORTHAMPTON Table, this rule would have appeared to be ſtill more incorrect than it does from thoſe computations.

When Mr. SIMPSON wrote his Select Exerciſes, he was in a great meaſure obliged to have recourſe to DE MOIVRE's hypotheſis, for want of thoſe excellent tables of the real probabilities of life, and alſo of the values of ſingle and joint lives which have been ſince publiſhed. Had he been poſſeſſed of theſe, it is moſt likely that his ſuperior abilities would have directed him to a more accurate method of inveſtigation. At preſent there can be no juſt reaſon for ever recurring to this wretched hypotheſis. The ſolutions of all caſes of two and even of three lives may be effected without much difficulty from principles ſtrictly true. But I muſt here take my leave of this ſubject, hoping that its importance may engage other mathematicians to the further proſecution of it.

* The true values are £. 66.038. and £. 74.884. reſpectively.

VI. *Result of Calculations of the Obſervations made at various Places of the Eclipſe of the Sun, which happened on* June 3, 1788. *By the Rev.* Joſeph Piazzi, *C. R. Profeſſor of Aſtronomy in the Univerſity of* Palermo; *communicated by* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read January 15, 1789.

TO DR. MASKELYNE.

SIR,

THE ſatisfaction I had in obſerving the eclipſe of the ſun on the third of June laſt, with you and M. D'ARQUIER, at Greenwich, induces me to give you an account of the uſe made of the obſervations, and the conſequences I have drawn from them. The obſervations which I have collected concerning the ſame eclipſe, and which were made in other places, contribute to the extenſiveneſs of my calculations, and to determine the poſition of certain places, which had not been before accurately determined, as that of Dublin, that of Mitau in Courland, and Perinaldo in Italy. The longitudes of all the other places muſt be referred to that of the Royal Obſervatory at Greenwich, as being the firſt in Europe, and becauſe the obſervations which you have made in it, are by far more accurate than any others made elſewhere.

The

The result of the observation at Greenwich is confirmed in the most satisfactory manner by the observations of Oxford and Loampit-Hill, which Dr. HORNSBY and Mr. AUBERT have done me the honour to communicate to me. These three observations perfectly agree in the latitude of the moon; whence it follows, that the duration of the eclipse was justly observed. And whereas the difference of longitude for Oxford, as determined by these observations, is only one second, and that of Loampit-Hill only two seconds, different from that which had been determined by means of the best time-keepers and other most exact observations, it follows, that these three observations may be considered as a single one, having a treble degree of accuracy. In effect, if the moment of conjunction found for Oxford and Loampit-Hill be reduced to the meridian of Greenwich, by adding to the latter 5″,4, and to the former 5′ 0″, and a mean be taken, it will appear, that this mean differs only by 0,6 of a second from the conjunction deduced from the observation made at Greenwich only. This also clearly proves, that the eclipses of the sun, when accurately observed, give nearly the same exactness as the occultations of the stars, which from their nature are considered as the most exact.

The observation made at Dublin deserves our greatest attention, particularly since the establishment of a very excellent observatory there. Dr. USSHER confesses, that the longitude of that city has not been exactly determined (see the Transactions of the Royal Irish Academy for the year 1787, p. 86.). He supposes the longitude of Dublin to be 24′ 58″ W. which he determined by means of a time-keeper, which Mr. ARNOLD happened to take with him to that city; whereas I find it to be 25′ 13″,4. This my determination I believe not to leave the uncertainty of two seconds;

ſeconds, becauſe the latitude of the moon deduced from the obſervation is the ſame with that of Greenwich.

The obſervation of Mitau is likewiſe very intereſting: it ſhews the ſituation of a diſtant country, where no obſervation had been made before M. BEITLER eſtabliſhed an obſervatory there. The obſervation of this able aſtronomer is of ſuch correctneſs, that it furniſhes the ſame latitude of the moon as the preceding ones; conſequently the difference of the meridians, which I thence deduce, may be conſidered to be as exact as that of Dublin. The difference is 1 h. 34′ 54″ E, which becomes of the greater importance to geography, becauſe from Pomerania to Peterſburg no one point had been accurately determined before.

The poſition of Berlin has been already determined by means of ſome eclipſes; but the reſults do not agree. The difference of 53′ 32″, which I have deduced, not only agrees exactly with that mentioned in Vol. IV. of M. DE LA LANDE's Aſtronomy, and which this famous aſtronomer had deduced from the occultation of Antares obſerved by himſelf in the year 1749; but it alſo comes ſo near to the longitude mentioned by Meſſ. LEXELL and BERNOULLI, in the Ephemeris of Berlin, as not to differ by more than two ſeconds.

The obſervation of Vienna gives for the difference of the meridians 1 h. 5′ 31″. Though this determination differs but 1″ from that found in the Almanack of Milan, and in the *Requiſite Tables*, yet the obſervations of the two phaſes had not been very accurately made.

Perinaldo in Italy is a place whoſe poſition has not been as yet well determined. The tables requiſite for the Nautical Ephemeris lay down this place at 30′ 40″ to the Eaſt of Greenwich; ſome place it at 30′ 20″. The obſervation made by

M. MARALDI, nephew of the Paris Academician, which I have by me, gives 30′ 53″ E. for the difference of the meridians, and this may be considered as the best hitherto known.

The observations made at Milan, by the astronomers DE CESARIS and REGGIO, were interrupted by intervening clouds. In fact, the latitude of the moon in conjunction comes out only equal to 14′ 32″, which shews that the duration of the eclipse was not properly observed. I have thence also calculated the conjunction separately for the beginning and for the end of the eclipse, and I have found out the following differences of meridians, *viz.* for the beginning 36′ 39″,6, and for the end 36′ 38″; and for the end and beginning conjointly 36′ 37″. This last difference comes nearest to that mentioned in the Milan Ephemeris for the year 1789, which is 36′ 41″. The observation made at Bologna assigns 45′ 28″ E. for the difference of the meridians. But the duration of the eclipse was not properly observed. However, notwithstanding this imperfection, it may happen that the result determined is exact.

The two observations of France, *viz.* that of Viviers, and that of Rouen, give almost the same difference which I find in the Requisite Tables; that of Rouen differing only 1″, and that of Viviers 2″. As the difference of the meridians between Paris and Rouen is known with the greatest precision to be 4′ 57″ to the W. of Paris; if to this difference are added 4′ 22″,3, which is the difference I found between Rouen and Greenwich, there will result, for the difference of the meridians between the Observatory of Greenwich and Paris, 9′ 19″,3. This difference only differs by 0″,7 from that established by Dr. BRADLEY, which is 9′ 20″, as adopted by yourself, and lately confirmed by Major-general ROY.

The

The obſervations of Loampit-Hill, Greenwich, and Oxford, as they ſerve for the baſis of all my calculations, I have calculated them two different ways, *viz.* by the method of parallactic angles, and by the method of the nonageſimal, and the reſults agreed together within a few tenths of a ſecond. By theſe two different methods I have alſo calculated the obſervations of Vienna, Berlin, and Viviers, in order to ſhew, that the different latitudes of the moon, given by the various obſervations, were not owing to any error in my calculations. For theſe places, in which both the beginning and the end of the eclipſe had been obſerved, I have deduced the time of conjunction from the two phaſes conjointly, which have alſo given the duration of the eclipſe, which cannot be obtained from a ſingle obſervation.

The error of the tables which reſults from the obſervation at Greenwich is $+26''$ in longitude, and $+11'',5$ in latitude, at 20 h. $58' 47'',3$ of apparent time, taking for the longitude of the ſun 2 s. $14° 16' 54'',7$, as I deduce from the Nautical Almanack, and that of the moon at the ſame time to be greater than the ſun by $26''$, as deduced from the ſame Almanack. I ſuppoſe alſo the horary motion of the moon in the ecliptic, by taking it an half hour before and after the conjunction, to be $36' 52'' + 0'',6$ for the hour following the conjunction, and $-0'',6$ for the hour preceding the conjunction; the moon's horary motion in latitude is $3' 24'',3$; the horizontal parallax of the moon *minus* that of the ſun at Greenwich, to be $60' 14'',4$ for the commencement of the eclipſe, and $60' 16'',4$ for the end; the ſun's diameter $31' 34'',6$, leſs by $3''$ than that given in the Almanack, according to the correction which you have found neceſſary to be made; the moon's diameter I have ſtated as in the Almanack. In the opinion of M. DE LA LANDE,

ſome correction ought to be made to the parallax and to the diameter of the moon, as well as to the diameter of the ſun; but on the one hand this would not make any alteration in the difference of the meridians which I have found; and on the other I thought proper to make uſe of thoſe elements the Nautical Almanack furniſhed me with, that being a work the moſt perfect of the kind that ever appeared, and to which all aſtronomers and navigators ought to pay the greateſt attention.

In fine, I compared the moon's longitude in conjunction deduced from the eclipſe with the new tables of the moor corrected by Mr. MASON, and found the longitude by thoſe tables to be 2 s. 14° 17′ 6″,4, and the latitude to be 15′ 1″,3. The error then of the new Tables is + 11″,7 in longitude, and + 13″,1 in latitude; but M. DE LA LANDE having lately ſent to me from Paris the place of the ſun, calculated with the new Solar Tables (a moſt uſeful improvement which M. DE LAMBRE has, with much ingenuity, deduced from your obſervations) I find the error in longitude to be + 27″,4, the ſun's place being 2 s. 14° 16′ 39″,0 at 20 h. 58′ 47″,3.

The following table contains the obſervations of the eclipſe, and the reſults deduced from thence. The firſt vertical column ſhews the name and place of the obſervers; the two next vertical columns contain all thoſe obſervations which have been made, in apparent time; the other columns ſhew the reſults, *viz.* the fourth column, contains the true conjunction in apparent time; the fifth column contains the longitude of the moon in conjunction, which being always the ſame, needs not to be repeated under every perpendicular column; the ſixth column contains the latitude of the moon, which, as it depends upon the manner of obſerving the two phaſes, is ſubject to

ſome variety; the ſeventh or laſt column contains the difference between the various meridians and that of Greenwich.

This, Sir, in brief, is the reſult which I have been able to deduce from the various obſervations above mentioned, and which I intirely ſubmit to your judgement. If you think that it deſerves to be made public, and in that caſe would be pleaſed to preſent this Paper to the Royal Society, I ſhall eſteem myſelf extremely honoured and obliged by it.

I have the honour to be, &c.

JOSEPH PIAZZI.

Table

Table of the observations made at various places on the eclipse of the sun, which happened June 3, 1788, and of results deduced from the same.

	Beginning.	End.	Conjunction.	Longitude of the moon in conjunction.	Latitude in conjunction	Difference of meridians.
Greenwich, Dr. MASKELYNE.	h. ′ ″ 19 24 46,5	h. ′ ″ 21 1 24,0	h. ′ ″ 20 58 47,3		′ ″ 14 48,2	″ 0
Loampit-Hill, Mr. AUBERT.	19 24 41,9	21 1 20,3	20 58 44,1		14 48,2	″ 3,2W
Oxford, Dr. HORNSBY.	19 20 36,1	20 54 40,0	20 53 46,2		14 48,7	′ ″ 5 1,1W
Dublin, Dr. USSHER.	19 5 46,5	20 27 42,1	20 33 33,9		14 48,3	25 13,4W
Mittau, M. BEITLER.	21 20 15,0	23 8 52,0	22 33 41,5		14 48,7	h. ′ ″ 1 34 54,2E
Berlin, M. BODE.	20 23 9,0	22 14 32,0	21 52 20,3		14 44,2	0 53 33E
Vienna, M. TRIESNEKER.	20 25 49,0	22 32 40,0	22 4 18,8	S. ° ′ ″ 2 14 16 54,7	14 39,0	1 5 31,5E
Viviers, M. FLAUGERGUAS.	19 26 38,0	21 25 41,0	21 17 29,0		14 33,0	′ ″ 18 41,7E
Perinaldo, M. MARALDI.	19 37 50,0	*	21 29 40		*	30 53,0E
Rouen, M. DU LAGNE.	*	21 7 15,0	21 3 9,6		*	4 22,3E
Milan, Meff. DE CESARIS and REGGIO.	19 48 23,0	21 51 14,0	21 35 24,7		14 32,0	36 37,4E
Bologna, M. MATTEUCCI.	19 55 10,5	22 3 45,5	21 44 15,3		14 31,0	45 28E
Padua, M. CHIMINELLO.	19 59 20,0	22 6 58,0	21 46 21,3		14 39,0	47 34E

POSTSCRIPT.

IN the month of February laſt, I was favoured by Count DE BRUHL with the obſervation of the eclipſe of the 4th of June laſt, made at Warſaw by M. BYSTRZYSKI; about the ſame time I alſo received of M. DE LA LANDE ſome other obſervations of the ſame eclipſe, *viz.* thoſe made at Prague, Marſeilles, Creſmunſter, and Bagdad in Meſopotamia, which I immediately calculated, in order to add them to the others, which Dr. MASKELYNE lately did me the honour of preſenting to the Royal Society.

The obſervation of Marſeilles confirms in the beſt manner the difference of meridians ſet down in the *Requiſite Tables*, differing from that only by a ſecond. The obſervation of Warſaw gives a difference ten ſeconds greater, and that of Creſmunſter fourteen ſeconds leſs; which differences ought not to ſurprize us, conſidering the obſervations upon which the longitudes of theſe two places had been eſtabliſhed; but, on the other hand, the obſervation of Prague clearly proves, that the ſituation of that town had been much leſs accurately determined than one might have expected. The time for the conjunction, which reſults from this obſervation, is the very ſame as that which is deduced by M. GERSTNER's new method, deſcribed in the Berlin Ephemeris for the year 1791, p. 243. From this time of conjunction the difference of meridians comes out equal to 57′ 42″,7, *viz.* one minute and ſeventeen ſeconds leſs that that of the Requiſite Tables.

The calculation of the obſervation made at Bagdat ſeems to indicate that there is ſome miſtake with regard to the end of the eclipſe, having found, that the difference of apparent lon-

gitude at the end is 20″ greater than the sum of the semidiameters of the sun and moon, increased in proportion of the apparent altitude of the moon: for this reason I do not give the moon's latitude in conjunction. As for the time of the conjunction, I deduce it both from the two phases together, and from the commencement only, having previously corrected the moon's latitude of the error which I discovered in the tables, *viz.* 11″,6. The time of conjunction which results from the first calculation is 23 h. 56′ 11″; that which results from the second 23 h. 56′ 16″: this last nearly agrees in the difference of meridians with the Ephemeris of Paris for the year 1789, and differs from the Requisite Tables by 2′ 32″.

The following table represents, as the first, the observations and the results.

	Beginning.	End.	Conjunction.	Longitude.	Latitude.	Difference of meridians.
Warsaw, M. BYSTRZYSKI	h. ′ ″ 20 56 45	h. ′ ″ 22 57 33	h. ′ ″ 22 22 59,3		′ ″ 14 44	h. ′ ″ 1 24 12
Prague, M. STRNADT.	20 21 29	22 21 15	21 56 30		14 45	0 57 42,7
Marseilles, M. BERNARD.	19 26 42	21 29 23,5	21 20 17,5		14 40	0 21 30,2
Cresmunster, M. FIXLMILLNER.	20 15 20	22 19 50,7	21 54 59		14 23	0 56 11,7
Bagdad, M. DE BEAUCHAMP.	22 30 51	23 26 19	23 56 11		*	2 57 23,7

VII. *An Account of a bituminous Lake or Plain in the Iſland of* Trinidad. *By Mr.* Alexander Anderſon; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read February 19, 1789.

A MOST remarkable production of nature in the iſland of Trinidad, is a bituminous lake, or rather plain, known by the name of Tar Lake; by the French called La Bray, from the reſemblance to, and anſwering the intention of, ſhip pitch. It lies in the leeward ſide of the iſland, about half-way from the Bocas to the ſouth end, where the Mangrove ſwamps are interrupted by the ſand-banks and hills; and on a point of land which extends into the ſea about two miles, exactly oppoſite to the high mountains of Paria, on the north ſide of the Gulf.

This cape, or head-land, is about fifty feet above the level of the ſea, and is the greateſt elevation of land on this ſide of the iſland. From the ſea it appears a maſs of black vitrified rocks; but, on a cloſe examination, it is found a compoſition of bituminous ſcoriæ, vitrified ſand, and earth, cemented together; in ſome parts beds of cinders only are found. In approaching this Cape, there is a ſtrong ſulphureous ſmell, ſometimes diſagreeable. This ſmell is prevalent in many parts of the ground to the diſtance of eight or ten miles from it.

This point of land is about two miles broad, and on the eaſt and weſt ſides, from the diſtance of about half a mile

from the sea, falls with a gentle declivity to it, and is joined to the main land on the south by the continuation of the Mangrove swamps; so that the bituminous plain is on the highest part of it, and only separated from the sea by a margin of wood which surrounds it, and prevents a distant prospect of it. Its situation is similar to a Savannah, and, like them, it is not seen till treading upon its verge. Its colour, and even surface, present at first the aspect of a lake of water; but I imagine it got the appellation of Lake when seen in the hot and dry weather, at which time its surface to the depth of an inch is liquid, and then from its cohesive quality it cannot be walked upon.

It is of a circular form, and I suppose about three miles in circumference. At my first approach it appeared a plane, as smooth as glass, excepting some small clumps of shrubs and dwarf-trees that had taken possession of some spots of it; but when I had proceeded some yards on it, I found it divided into areolæ of different sizes and shapes: the chasms or divisions anastomosed through every part of it; the surface of the areolæ perfectly horizontal and smooth; the margins undulated, each undulation enlarged to the bottom till they join the opposite. On the surface the margin or first undulation is distant from the opposite from four to six feet, and the same depth before they coalesce; but where the angles of the areolæ oppose, the chasms or ramifications are wider and deeper. When I was at it, all these chasms were full of water, the whole forming one true horizontal plane, which rendered my investigation of it difficult and tedious, being necessitated to plunge into the water a great depth in passing from one areola to another. The truest idea that can be formed of its surface will be from the areolæ and their ramifications on the back of

a turtle. Its more common conſiſtence and appearance is that of pit-coal, the colour rather greyer. It breaks into ſmall fragments, of a cellular appearance and gloſſy, with a number of minute and ſhining particles interſperſed through its ſubſtance; it is very friable, and, when liquid, is of a jet black colour. Some parts of the ſurface are covered with a thin and brittle ſcoria, a little elevated.

As to its depth, I can form no idea of it; for in no part could I find a ſubſtratum of any other ſubſtance; in ſome parts I found calcined earth mixed with it.

Although I ſmelt ſulphur very ſtrong on paſſing over many parts of it, I could diſcover no appearance of it, or any rent or crack through which the ſteams might iſſue; probably it was from ſome parts of the adjacent woods: for although ſulphur is the baſis of this bituminous matter, yet the ſmells are very different, and eaſily diſtinguiſhed, for its ſmell comes the neareſt to that of pitch of any thing I know. I could make no impreſſion on its ſurface without an axe: at the depth of a foot I found it a little ſofter, with an oily appearance, in ſmall cells. A little of it held to a burning candle makes a hiſſing or cracking noiſe like nitre, emitting ſmall ſparks with a vivid flame, which extinguiſhes the moment the candle is removed. A piece put in the fire will boil up a long time without ſuffering much diminution: after a long time's ſevere heat, the ſurface will burn and form a thin ſcoria, under which the reſt remains liquid. Heat ſeems not to render it fluid, or occupy a larger ſpace than when cold; from which, I imagine, there is but little alteration on it during the dry months, as the ſolar rays cannot exert their force above an inch below the ſurface. I was told by one Frenchman, that in the dry ſeaſon the whole was an uniform ſmooth maſs; and by another, that the ravins

contained water fit for use during the year; but neither can I believe: for if, according to the first assertion, it was an homogeneous mass, something more than an external cause must affect it, to give it the present appearances: nor without some hidden cause can the second be granted. Although the bottoms of these ramified channels admit not of absorption, yet from their open exposure, and the black surface of the circumjacent parts, evaporation must go on amazing quick, and a short time of dry weather must soon empty them; nor from the situation and structure of the place is there a possibility of supply but from the clouds. To shew that the progress of evaporation is inconceivably quick here, at the time I visited it, there were, on an average, two-thirds of the time incessant torrents of rain; but from the afternoon being dry, with a gentle breeze (as is generally the case during the rainy season in this island), there evidently was an equilibrium between the rain and the evaporation; for in the course of three days I saw it twice, and perceived no alteration on the height of the water, nor any outlet for it but by evaporation.

I take this bituminous substance to be the *bitumen asphaltum* LINNÆI. A gentle heat renders it ductile; hence, mixed with a little grease or common pitch, it is much used for the bottoms of ships, and for which intention it is collected by many, and I should conceive it a preservative against the Borer, so destructive to ships in this part of the world.

Besides this place, where it is found in this solid state, it is found liquid in many parts of the woods; and at the distance of twenty miles from this about two inches thick, round holes of three or four inches diameter, and often at cracks or rents. This is constantly liquid, and smells stronger of tar than when indurated,

indurated, and adheres ſtrongly to any thing it touches; greaſe is the only thing that will diveſt the hands of it.

The ſoil in general, for ſome diſtance round La Bray, is cinders and burnt earths; and where not ſo, it is a ſtrong argillaceous ſoil; the whole exceedingly fertile, which is always the caſe where there are any ſulphureous particles in it. Every part of the country, to the diſtance of thirty miles round, has every appearance of being formed by convulſions of nature from ſubterraneous fires. In ſeveral parts of the woods are hot ſprings; ſome I tried, with a well graduated thermometer of FAHRENHEIT, were 20° and 22° hotter than the atmoſphere at the time of trial. From its poſition to them, this part of the iſland has certainly experienced the effects of the volcanic eruptions, which have heaped up thoſe prodigious maſſes of mountains that terminate the province of Paria on the north; and no doubt there has been, and ſtill probably is, a communication between them. One of theſe mountains oppoſite to La Bray in Trinidad, about thirty miles diſtant, has every appearance of a volcanic mountain: however, the volcanic efforts have been very weak here, as no trace of them extend above two miles from the ſea in this part of the iſland, and the greater part of it has had its origin from a very different cauſe to that of volcanos; but they have certainly laid the foundation of it, as is evident from the high ridge of mountains which ſurrounds its windward ſide to protect it from the depredations of the ocean, and is its only barrier againſt that over-powering element, and may properly be called the ſkeleton of the iſland.

From every examination I have made, I find the whole iſland formed of an argillaceous earth, either in its primitive ſtate, or under its different metamorphoſes. The baſes of the mountains

mountains are compoſed of *ſchiſtus argillaceus* and *talcum lithomargo*; but the plains or low-lands remaining nearly in the ſame moiſt ſtate as at its formation, the component particles have not experienced the viciſſitudes of nature ſo much as the more elevated parts, conſequently retain more of their primitive forms and properties. As argillaceous earth is formed from the ſediment of the Ocean, from the ſituation of Trinidad to the Continent, its formation is eaſily accounted for, granting firſt the formation of the ridge of mountains that bound its windward ſide, and the high mountains on the Continent that nearly join it: for the great influx of currents into the Gulf of Paria from the coaſts of Brazil and Andaluſia muſt bring a vaſt quantity of light earthy particles from the mouths of the numerous large rivers which traverſe theſe parts of the Continent; but the currents being repelled by theſe ridges of mountains, eddies and ſmooth water will be produced where they meet and oppoſe, and therefore the earthy particles would ſubſide, and form banks of mud, and by freſh accumulations added would ſoon form dry land; and from theſe cauſes it is evident ſuch a tract of country as Trinidad muſt be formed. But theſe cauſes ſtill exiſt, and the effect from them is evident; for the iſland is daily growing on the leeward ſide, as may be ſeen from the mud-beds that extend a great way into the Gulf, and there conſtantly increaſe. But from the great influx from the Ocean at the ſouth end of the iſland, and its egreſs to the Atlantic again, through the Bocas, a channel muſt ever exiſt between the Continent and Trinidad.

VIII. *An Account of a particular Change of Structure in the human Ovarium.* By Matthew Baillie, *M. D.*; *communicated by* John Hunter, *Esq. F. R. S.*

Read February 26, 1789.

THE ovaria in women are subject to a great variety of changes from their natural structure. Many of these are exactly similar to what take place in other parts of the body; but there is one which seems peculiar to them, the nature of which has probably not been hitherto very well ascertained. The change of structure to which I allude, is a conversion of the natural substance of an ovarium into a fatty mass, intermixed with hair and teeth. This sort of change is rare, although it occurs sufficiently often to have been seen by most persons who are very conversant in the examination of dead bodies. There are many cases of it related in the different books of dissections, but, as far as I have discovered, most commonly without any remarks upon the mode of formation *; or they have been considered as very imperfect attempts at the

* It has been the opinion of some, that hair, teeth, nails, feathers, &c. are animal vegetables or plants; and, agreeably to this opinion, Dr. Tyson considers the growth of hair and teeth in the ovarium as a *lusus naturæ*, where nature endeavours to produce something, and being disappointed in forming an animal, produces a vegetable. Vide Hooker's Lectures and Collection, N. II. p. 11, and 15.

growth

growth of a fœtus in the ovarium, in conſequence of connection between a male and a female. This conjecture reſts no doubt on ſtrong circumſtances of probability, and yet there are many powerful reaſons which ſeem to oppoſe its being well founded. Generation is a proceſs always depending on the action of a certain cauſe, *viz.* the uſual connection between a male and a female; and, when effects ſimilar to thoſe in generation are perceived, it becomes very natural to conclude, that this cauſe has been employed. The bias to ſuch an opinion will become the ſtronger, from reflecting on the paſſions that are known to influence ſo powerfully mankind, by which the agency of this cauſe is frequently excited. When a change, therefore, was obſerved in an ovarium, by which it was converted into a fatty maſs with hair and teeth, this ſhould ſeem to correſpond ſo much with a change taking place in conſequence of generation, that the mind would ſcarcely entertain a doubt of its ariſing from the ſame cauſe, and would readily infer, that it had been preceded by a connection between the ſexes. This doubt would ſtill be the leſs, from the circumſtance of a complete fœtus being ſometimes formed in the ovarium, where the uſual means of generation had been employed. The following caſe, however, exhibits many reaſons why we ſhould be led to believe, that the ovaria in women have ſome power within themſelves of taking on a proceſs which is imitative of generation, without any previous connection with a male; and it is with this view that I proceed to relate it.

In a female child, about twelve or thirteen years old, which was lately brought to Windmill-ſtreet for diſſection, I found the right ovarium converted into a ſubſtance, doughy to the touch, and about the ſize of a large hen's egg. Upon cutting into the ſubſtance, I found an apparently fatty maſs, intermixed

mixed with hair and an excreſcence of bones. This ſtartled me very much, as I had always been led to believe, that ſuch appearances were a ſort of imperfect conception. The circumſtances altogether being very ſingular, I was led to pay conſiderable attention to the change in the ovarium.

The fatty maſs was of a yellowiſh white colour, in ſome places more yellow than in others, was very unctuous to the feeling, and conſiſted of ſhortened or ſeparated particles, not having the ſame coaleſcence which the fat has generally in the body. It became very ſoft when expoſed to the heat of a fire, and ſunk into a portion of paper, on which it was ſpread, ſo as to make it more tranſparent. When the paper to which it was applied was expoſed to the flame of a candle, it burnt with conſiderable crackling.

The hair with which the fatty ſubſtance was mixed grew out of the inner ſurface of the capſule containing it, in ſome places in ſolitary hairs, but chiefly in ſmall faſciculi, at ſcattered irregular diſtances. Beſides theſe, there were looſe hairs involved in the fatty maſs. The hairs were, ſome of them, of conſiderable length, even to three inches, were fine, and of a light-brown colour. They reſembled much more the hairs of the head, than what are commonly found on the pubis, and correſponded very much in colour to the hair of the girl's head.

There aroſe alſo from the inner ſurface of the capſule ſome veſtiges of human teeth. One appeared to be a canine tooth, another to be a ſmall grinder, two others to be inciſors, and there was alſo a very imperfect attempt at the formation of another tooth. Theſe were not fully formed, the fangs being wanting; but in two of them the bodies were as complete as they are ever found in the common circumſtances. They were each

of them incloſed in a proper capſule, which aroſe from the inner ſurface of the ovarium, and conſiſted of a white thick opaque membrane. Attached to the capſules of three of the teeth, there was a white ſpungy ſubſtance. The membrane of the ovarium itſelf was of ſome conſiderable thickneſs, but unequal in the different parts, was very ſmooth in its inner ſurface, and more irregular externally. The uterus was ſmaller than it is commonly at birth, was perfectly healthy in its ſtructure, and upon opening into its cavity it exhibited the ordinary appearances of a child's uterus at that period. The left ovarium was very ſmall, correſponding to the ſtate of the uterus. It appears clearly from this, that the uterus had not yet received the increaſe of bulk, which is uſual at the age of puberty. The hymen was entire, ſuch as is commonly found in a child of the ſame age; and there was juſt beginning a *lanugo* upon the labia, not more than what is often found on the upper lip of a boy of fifteen years old. Such are the circumſtances attending this ſingular caſe, and they preſent to the mind various grounds of conſideration.

The formation of hair and teeth is a ſpecies of generation, for in fact it makes a part of it, and ſtrikes the mind as being very different from any irregular ſubſtance which is formed by diſeaſe. This formation too takes place in a part of the body which is ſubſervient to generation, and where a complete fœtus is ſometimes formed. The whole of this looks very much as if the production of hair and teeth in the ovarium was a ſort of imperfect impregnation. But when we take another view of it, there are reaſons at leaſt equally ſtrong for believing that ſuch productions may ariſe from an action in the ovarium itſelf, without any ſtimulus from the application of the male ſemen.

In

In the case before us, the uterus was as small as at birth, indeed more so, and the left ovarium (which was perfectly healthy) corresponded to the state of the uterus. It had not been at all stimulated, nor did appear capable of being stimulated by the application of the male semen. This seems to be a strong circumstance; for in a case where there was an ovum formed in one of the Fallopian tubes, the uterus was enlarged to more than twice its unimpregnated size; and, upon opening into its cavity, the decidua was observed to be formed as completely as in the impregnated uterus. This preparation is still preserved in the collection of Windmill-street. Nothing can be a stronger proof, that when an impregnation takes place out of the cavity of the uterus, the uterus still takes a share in the action, and undergoes some of the changes of impregnation. In another preparation, which is preserved in the same collection, where there was a fœtus formed in the ovarium, the uterus was increased to more than twice its ordinary size, was very thick and spungy, and had its blood-vessels enlarged as in an impregnated uterus. This becomes another very strong proof of the action of the uterus in the formation of an extra-uterine fœtus. In the case before us, however, the uterus had undergone no change, and does not seem to have arrived at that period, when it could be capable of undergoing such a change.

Besides, we are not to consider the formation of teeth in the ovarium to be a quicker process than it is commonly in the head of a fœtus; but in the present case the teeth having advanced fully as far as they are at some months after birth, this process must have begun at least more than a twelvemonth before the death of the child. If then we consider it as an impregnation, since the appearances of the child do not warrant us to believe her to have been more than twelve or thirteen years

old, this brings the date of the impregnation to an earlier period than can well be believed. From all theſe circumſtances we might be led to ſuppoſe, that the formation of the hair and teeth was not in conſequence of any connection with a male, but aroſe from ſome action of the ovarium itſelf, in which the uterus did not participate. The exiſtence of the hymen, eſpecially in ſo young a girl, becomes a collateral confirmation of the ſame opinion, although much is not to be reſted on it, when taken ſingly.

It will, perhaps, have ſome influence in removing the prejudices againſt this opinion, to make the following remarks. Hair is occaſionally formed in parts of the human body, which are abſolutely unconnected with generation. Encyſted tumours are ſometimes found containing hair. Mr. HUNTER has a preparation of this ſort in his collection, which he cut out from under the ſkin of the eyebrow. This tumour was perfectly complete, and unconnected with the ſkin, except by the common intervention of cellular membrane, ſo as to have no communication whatever with the hair of the eyebrow. In this inſtance there was certainly a ſpecies of generation taking place in the encyſted tumour itſelf, forming hairs as completely and fully as in the common progreſs of the formation of a child. Such encyſted tumours have been found in other parts of the human body, and ſtill more frequently in quadrupeds. Mr. HUNTER has in his collection many ſpecimens of encyſted tumours from cows and ſheep containing hair and wool. Theſe were perfectly complete, ſo as to have poſſeſſed a power of production within themſelves, and were many of them found deeply ſeated at a conſiderable diſtance from the ſkin, which is the common parent of hair. In theſe tumours there is often the appearance of layers of cuticle, which is

probably a preparatory ſtep to the formation of hair. All this ſhews moſt clearly, that hair may be formed without any ſpecies of generation as it is commonly underſtood.

But hair is in itſelf as diſtinct a conſequence of generation as teeth, and as much a peculiar ſubſtance. If then the one be formed, there appears to be no reaſon why the other ſhould not alſo be formed. The action producing the one is not better underſtood than that producing the other; nor does it appear to be really in itſelf leſs connected with that ſpecies of generation ariſing from the approach of a male, ſo that teeth may probably be formed by a peculiar action taking place in the ovarium itſelf, as well as the hair.

It will tend to add further weight to this opinion, to conſider that many of the adult teeth are formed in a child after birth; and therefore their formation depends on an action taking place in the jaws at a particular period, and not on original growth. The ſame circumſtance ſtrikes more ſtrongly in the occaſional formation of teeth at an advanced time of life. Both of theſe proceſſes take place after the animal has been formed, in conſequence of a certain action being excited in a particular part of the body, and therefore there is leſs difficulty in believing that the ſame ſort of proceſs may go on in another part of the body not commonly employed in it. It ſeems reaſonable alſo to ſuppoſe, that the ovaria ſhould have a greater aptitude of taking on a proceſs ſomewhat ſimilar to generation than the other indifferent parts of the body, as they conſtitute a part of the organs which are ſo materially concerned in the real proceſs itſelf *. Theſe circumſtances, when taken collectively, would

* As the formation of teeth and hair involved in a fatty maſs may be ſaid to be peculiar to the ovaria, and as there are ſtrong reaſons for believing, that this formation

would ſeem to render it very probable, that the formation of hair and teeth in the ovarium does not neceſſarily depend on a connection between a male and a female (as has been the common opinion), but ariſes from ſome action within the ovarium itſelf, which is imitative of generation.

formation may take place without an intercouſe between the ſexes, it becomes difficult to account for this peculiarity in them, unleſs by ſuppoſing, that they have a greater aptitude of running into ſuch a proceſs than the other parts of the body.

IX. *Some Account of the Vegetable and Mineral Productions of* Boutan *and* Thibet. *By Mr.* Robert Saunders, *Surgeon at* Boglepoor *in* Bengal; *communicated by Sir* Joſeph Banks, *Bart. P. R. S.*

Read February 26 and March 5, 1789.

ROAD to Buxaduar, May 11 and 12, 1783. The tract of country from Bahar to the foot of the hills contains but few plants that are not common to Bengal. Pine-apples, mango-tree, jack and ſaul timber, are frequently to be met with in the foreſts and jungles. Find many orange-trees towards the foot of the hills, of a very good ſort, and bearing much fruit. Saw a few lime-trees, and found three different ſpecies of the ſenſitive plant. One ſpecies is uſed medicinally by the natives of Bengal in fevers; it is a powerful aſtringent and bitter; another is the ſpecies from which Terra Japonica is made, a medicine the hiſtory of which we are but lately made acquainted with. The third ſpecies is well known as the ſenſitive plant, and common in Bengal.

The country, from Bahar to the foot of the mountains, to which we approach without any aſcent, is rendered one of the moſt unhealthy parts of India, from a variety of cauſes.

The whole, a perfect flat, is at all times wet and ſwampy, with a luxuriant growth of reeds, long graſs, and underwood, in the midſt of ſtagnated water, numerous frogs and inſects. The exhalations from ſuch a ſurface of vegetable matter and ſwamps, increaſed by an additional degree of heat from the reflection

reflection of the hills, affect the air to a conſiderable extent, and render it highly injurious to ſtrangers and European conſtitutions.

The thermometer at the foot of the hill, mid-day 86°, fell to 78° at two o'clock, the time we reached Buxaduar, and that hour of the day when it is generally higheſt.

The ſoil and appearance of the ground in aſcending the hill are materially changed. See many looſe ſparry ſtones and rock containing iron. Two ſprings, conducted from a diſtant height by ſpouts, are very pure and good water, without any mineral impregnation. The mountains in view covered with foreſts of trees, rendered uſeleſs from their inacceſſibility. Thoſe peculiar to the country are known to the natives by the names Boumbſhi, Toumbſhi and Sindeſhi, beſides ſaul timber, bamboo, and plantains.

Buxaduar, May 12 to 21. Many of the plants peculiar to Bengal require nurſing at Buxaduar. There is one very good banian tree. In the jungles, met with the ginger, and a very good ſort of yam; ſaw ſome pomegranate-trees in good preſervation; ſhallots in great perfection; a ſpecies of the Lychnis, Arum, and Aſclepias, natives of more northern ſituations, and of little uſe; a bad ſort of raſberry, and a ſpecies of the Glorioſa. The plantains in uſe below do not thrive here. In the jungles they have a plantain-tree producing a very broad leaf, with which they cover their huts; but the fruit is not eaten. See many weeds and long graſs more common to Bengal than any other parts of Boutan.

From the 15th to the 22d, the rains were almoſt inceſſant at Buxaduar. Our People became unhealthy, and were attacked with fevers, which, if neglected in the beginning, proved obſtinate quartans. This was the caſe with ſeveral of the

the natives whom I had an opportunity of ſeeing. They ſcarcely, however, admit that Buxaduar is unhealthy at any ſeaſon of the year. After allowing for their prejudice, and the poſſibility of the natives ſuffering but little from the bad ſeaſons, I cannot help thinking, that Buxaduar muſt be unhealthy, at leaſt to ſtrangers, from the month of May till towards the end of September. It lies high, but is overtopped by the ſurrounding mountains, covered with foreſts of trees and underwood. In all climates, where the influence of the ſun is great, this is a never-failing cauſe of bad air. The exhalation that takes place from ſo great a ſurface in the day-time falls after ſunſet in the form of dew, rendering the air raw, damp, and chilly, even in the moſt ſultry climates.

The thermometer at Buxaduar was never, at two o'clock in the afternoon, above 82°, or below 73°.

In the neighbourhood of Buxaduar there are ſeveral excellent ſprings of water, ſome of them with leſs impregnation of any ſort than I ever met with; the niceſt teſt ſcarcely produced the ſeparation of a ſenſible quantity of earthy matter. Such waters are generally to be diſtinguiſhed by the taſte, which is inſipid and unpleaſant. When theſe ſprings could be traced to their ſource, they ſunk the thermometer eight or ten degrees below the temperature of the atmoſphere.

Road to Muriſhong, May 22 and 23. In aſcending the hill from Buxaduar there is to be ſeen much of an imperfect quartz, of various forms and colour, having in ſome places the appearance of marble; but from chemical experiments, it was found to poſſeſs very different properties. This ſort of quartz, when of a pure white, and free from any metallic colouring matter, is uſed as an ingredient in porcelain. I have not ſeen any that promiſes to anſwer that purpoſe better than what is to be met with in the mountains near

Buxaduar. It is known to mineraliſts in that ſtate by the name of quartz gritſtone. The rock which forms the baſis of theſe mountains dips in almoſt every direction, and is covered with a rich and fertile ſoil, but in no place level enough to be cultivated. Many European plants are met with on the road to Muriſhong; many different ſorts of moſſes, fern, wild thyme, peaches, willow, chickweed, and graſſes common to the more ſouthern parts of Europe, nettles, thiſtles, dock, ſtrawberry, raſberry, and many deſtructive creepers, ſome peculiar to Europe.

Muriſhong is the firſt pleaſant and healthy ſpot to be met with on this ſide of Boutan. It lies high, and much of the ground about it is cleared and cultivated; the ſoil, rich and fertile, produces good crops. The only plant now under culture is a ſpecies of the Polygonum of LINNÆUS, producing a triangular ſeed, nearly the ſize of barley, and the common food of the inhabitants. It was now the beginning of their harveſt; and the ground yields them, as in other parts of Boutan, a ſecond crop of rice. Here are to be found in the Jungles two ſpecies of the Laurus of LINNÆUS; one known by the name of the baſtard cinnamon. The bark of the root of this plant, when dried, has very much the taſte and flavour of cinnamon; it is uſed medicinally by the natives. The Chenopodium, producing the ſemen ſantonicum, or worm-ſeed, a medicine formerly in great character, and uſed in thoſe diſeaſes from which it is named, is common here.

Found in the neighbourhood of this place all the European plants we had met with on the road. The aſcent from Buxaduar to Muriſhong is upon the whole great, with a ſenſible change in the ſtate of the air.

Road to Chooka, May 25. On the road to Chooka find all the Muriſhong plants, cinnamon-tree, willow, and one or two firs;

firs; ſtrawberries every where, and very good, and a few bilberry plants.

Much ſparry flint, and a ſort of granite with which the road is paved. There is a great deal of talc in the ſtones and ſoil, but in too ſmall pieces to be uſeful. Frequent beds of clay and pure ſand. Find two mineral wells, ſlightly impregnated with iron, with much appearance of that metal in this part of the country; and they are not unacquainted with the method of extracting it from the ſtones, but ſtill deſpiſe its uſe in building. Towards Chooka there are many well cultivated fields of wheat and barley.

Road to Punukha, May 26. From Chooka the country opens, and preſents to view many well cultivated fields and diſtant villages; a rapid change in climate, the vegetable productions, and general appearance of the country. Towards Punukha, pines and firs are the only trees to be met with; but they do not yet ſeem in their proper climate, being dwarfiſh and ill-ſhaped; peaches, raſberries, and ſtrawberries, thriving every where; ſcarce a plant to be ſeen that is not of European growth. In addition to the many I have already mentioned, ſaw two ſpecies of the Cratægus, one not yet deſcribed. Saw two aſh-trees in a very thriving ſtate, the ſtar-thiſtle, and many other weeds, in general natives of the Alps and Switzerland.

Much of the rock to-day is, I find on examination, pure limeſtone; a valuable acquiſition if they did not either deſpiſe its uſe, or were unacquainted with its properties. It was moſt advantageouſly ſituated for being worked, and the pureſt perhaps to be met with. There is likewiſe abundance of fire-wood in this part of the country. In building they would derive great benefit from the uſe of it. Their houſes are lofty, the timbers ſubſtantial, and nothing wanting

to make them durable, but their being acquainted with the uſe of lime. As a manure it might probably be uſed to great advantage. Many fields of barley in this part of the country; now the beginning of their harveſt. The thermometer here fell, at four o'clock in the afternoon, to 60°: cold and chilly.

Road to Chepta, May 27. On the road to Chepta, the rock in general dips to the northward and eaſtward, in about an angle of ſixty degrees. Much of limeſtone, and ſome veins of quartz, and looſe pieces of ſparry flint ſtriking fire with ſteel.

Several ſprings, and one ſlightly impregnated with iron.

In addition to the plants of yeſterday, find the Coriandrum teſticulatum, Inula montana, and Rhododendum magnum.

At Chepta met with a few turneps, one maple-tree, wormwood, gooſe-graſs (Galium aparine), and many other European weeds; the firſt walnut-tree we had yet ſeen.

Chepta lies high, and not above ſix miles from the mountain of Lomyla, now covered with ſnow. The wind from that quarter, S.E. made it cold and chilly, and ſunk the thermometer at mid-day to 57°. Here are ſome fields of wheat and barley not yet ripe.

Road to Pagha, May 29. Soon after leaving Chepta find a mineral well, which, on a chemical examination, gave marks of a ſtrong impregnation from iron. I traced it to its ſource, where the thermometer, on being immerſed, fell from 68° to 56°.

A little before we reach Pagha, met with ſome limeſtone, and a bed of chalk, which, near the ſurface, contained a great proportion of ſand, but ſome feet under was much purer.

The

The foreſts of firs of an inferior growth, ſeveral aſh-trees, dog-roſe, and bramble.

Road to Taſſeſudon, May 30 and 31, June 1. The road from here to Taſſeſudon preſents us with little that we have not met with; fewer ſtrawberries, and no raſberries; ſome very good orchards of peaches, apricots, apples, and pears. The fruit formed, and will be ripe in Auguſt and September. Met with two ſorts of cranberry, one very good. Saw the Fragaria ſterilis, and a few poppies. At Wanakha found a few turneps, ſhallots, cucumbers, and gourds. Near to Taſſeſudon the road is lined with many different ſpecies of the roſe, and a few jeſſamine plants. The ſoil is light, and the hills in many places barren, rocky, and with very little verdure. The rock in general laminated and rotten, with many ſmall particles of talc in every part of the country incorporated with the ſtones and ſoil. Some limeſtone, and appearance of good chalk. Several good and pure ſprings of water.

Taſſeſudon and its neighbourhood abound with all the plants we have already mentioned. The hills are chiefly wood, with firs and aſpen. I have not yet been able to find an oak-tree, and the aſh is very ſeldom to be met with. The elder, holly, bramble, and dog-roſe, are common. Found the birch-tree, cypreſs, yew, and delphinium. Many different ſpecies of the vaccinium, of which the bilberry is one, and the cranberry another. Towards the top of the adjacent mountains met with two plants of the Arbutus uva urſi, which is a native of the Alps, the moſt mountainous parts of Scotland, and Canada.

I have likewiſe ſeen a ſpecies of the rhubarb plant (Rheum undulatum) brought from a diſtance, and only to be met with near the ſummits of hills covered with ſnow, and where the

ſoil

ſoil is rocky. The true rhubarb (Rheum palmatum) is likewiſe the native of a cold climate; and though China ſupplies us with much of this drug, it is known to be the growth of its more northern provinces, Tartary, and part of the Ruſſian dominions. The great difficulty is in drying the root. People verſant in that buſineſs ſay, that one hundred pounds of the freſh root ſhould not weigh above ſix pounds and a half, if properly dried, and it certainly has been reduced to that. I have ſeen eighty pounds of freſh root produced from one plant; but, after drying it with much care and attention, the weight of the dried root could not be made leſs than twelve pounds. It was ſuſpended in an oven, with an equal and moderate degree of heat. Little more than the ſame quantity of this powder produced a ſimilar effect with the beſt foreign rhubarb.

The other plants common here are the ſervice-tree, bleſſed thiſtle, mock orange, Spiræa filipendula, Arum, Echites, Punica, Ferula communis, Erica, and Viola. Of the roſe-buſh I have met with the five following ſpecies; Roſa alpina, centifolia, canina, Indica, ſpinoſiſſima.

The culture of pot-herbs is every where neglected; turneps, a few onions and ſhallots, were the beſt we could procure. Mr. BOGLE left potatoes, cabbage, and lettuce-plants, all which we found neglected and diſperſed. They had very improperly (from an idea moſt probably of their being natives of Bengal) planted them in a ſituation and climate which approaches very near to that of Bengal at all ſeaſons, as we ſhall find afterwards. Melons, gourds, brinjals, and cucumbers, are occaſionally to be met with. The country is fitted for the production of every fruit and vegetable common without the tropics, and in ſome ſituations will bring to perfection many of the tropical fruits.

There are two plants which I have to regret the not having had as yet an opportunity of ſeeing; one is the tree from the bark of which their paper is made; and the other is employed by them in poiſoning their arrows. This laſt is ſaid to come from a very remote part of the country. They deſcribe it as growing to the height of three or four feet, with a hollow ſtalk. The juice is inſpiſſated, and laid as a paſte on their arrows. Fortunately for them, it has not all the bad effects they dread from it. I had an opportunity of ſeeing ſeveral who were wounded with theſe arrows, and they all did well, though under the greateſt apprehenſion. The cleaning and enlarging ſome of the wounds was the moſt that I found neceſſary to be done. The paſte is pungent and acrid, will increaſe inflammation, and may make a bad or neglected wound mortal; but it certainly does not poſſeſs any ſpecific quality as a poiſon.

The fir, ſo common in this country, is perhaps the only tree they could convert to a uſeful and profitable purpoſe. What I have ſeen would not, from their ſituation, be employed as timber. The largeſt I have yet met with were near Wandepore; they meaſured from eight to ten feet in circumference, were tall and ſtraight. Such near the Burrampooter, or any navigable river, might certainly be tranſported to an advantageous market. I am convinced that any quantity of tar, pitch, turpentine, and reſin, might be made in this country, much to the emolument of the natives. Firs, which from their ſize and ſituation are unfit for timber, would anſwer the purpoſe equally well. The proceſs for procuring tar and turpentine is ſimple, and does not require the conſtruction of expenſive works. This great object has been ſo little attended to, that they are ſupplied from Bengal with what they want of theſe articles.

The

The country about Taſſeſudon contains great variety of ſoil, and much rock of many different forms, but ſtill an unpromiſing field for a mineraliſt. I have not found in Boutan a foſſil that had the leaſt appearance of containing any other metal than iron, and a ſmall portion of copper. From information, and the reports of travellers, I believe it is otherwiſe to the northward. The banks of the Ticuſhu, admitting of cultivation for ſeveral miles above and below Taſſeſudon, yield them two crops in the year. The firſt of wheat and barley is cut down in June; and the rice, planted immediately after, enjoys the benefit of the rains. This country is not without its hot wells, as well as many numerous ſprings, ſome of which I have taken notice of. One hot well, near Wandepore, is ſo cloſe to the banks of the river as to be overflowed in the rains, and we found it impoſſible to get to it: the heat of this well is great; but I could not learn that the ground about it was much different from the general aſpect of the country. Another, ſeveral days journey from hence, is on the brow of a hill perpetually covered with ſnow. This hot well is held in great eſtimation by the people of the country, and reſorted to by valetudinarians of every deſcription. I gained but little ſatisfactory information reſpecting the degree of heat, or appearance of the ground about it, that could lead me to form a juſt opinion of either.

Taſſeſudon to Paraghon, Sept. 8 and 9. Left Taſſeſudon, and arrived next day at Paraghon. Much good rich ſoil, with more paſture, where the ground is not cultivated, than we had yet met with. Many fields of turneps in great perfection; a plant they ſeem better acquainted with the cultivation of than any other. Find on the road many large and well-thriving birch, willows, pines, and firs, ſome walnut-trees, the

Arbutus

Arbutus uva urſi, abundance of ſtrawberry, elderberry, bilberry, Chryſanthemum or greater daiſy, and many European graſſes. See the Datura ferox or thorn-apple, a plant common in China and ſome parts of Thibet, where it is uſed medicinally. They find it a powerful narcotic, and give the ſeeds where they wiſh that effect to be produced. It has been uſed as a medicine in Europe, and is known to poſſeſs theſe qualities in a high degree. See holly, dog-roſe, and aſpen. The preſent crop near Paraghon, on the banks of the Pachu, is rice, but not ſo far advanced as at Taſſeſudon; the ſame may be ſaid of their fruits. They ſay it is colder here at all ſeaſons than at Taſſeſudon, which is certainly below the level of this place.

Towards the ſummit of the mountain we croſſed, found ſome rock of a curious appearance, forming in front ſix or ſeven angular ſemi-pillars, of a great circumference, and ſome hundred feet high. This natural curioſity was detached in part from the mountain, and projected over a conſiderable fall of water, which added much to the beautiful and pictureſque appearance of the whole. Numerous ſprings, ſome degrees colder than the ſurrounding atmoſphere, guſhing from the rock on the moſt elevated part of the mountain, furniſh a very ample and ſeaſonable ſupply of excellent water to the traveller. The rock, in many places laminated, might be formed into very tolerable ſlate. Near to Paraghon iron ſtones are found, and one ſpring highly impregnated with this mineral.

Road to Dukaigun, Sept. 11. Our road to Dukaigun, nearly due north, is a continued aſcent for eight miles, along the banks of the Pachu, falling over numerous rocks, preci-

pices, and huge ſtones. Here we begin to experience a very conſiderable change in the temperature of the atmoſphere; the ſurrounding hills were covered with ſnow in the morning, which had fallen the preceding night, but diſappeared ſoon after ſunriſe. The thermometer fell to 54° in the afternoon, and did not riſe above 62° at noon.

The face of the mountains, in ſome places bare, with projecting rock of many different forms; quartz, flint, and a bad ſort of freeſtone, common. Many very good ſprings, ſlightly impregnated with a ſelenitic earth.

The ſoil is rich, and near to the river in great cultivation. Many horſes, the ſtaple article of their trade, are bred in this part of the country. Found walnut-trees, peaches, apples, and pears.

Road to Sanha, Sept. 12. The road ſtill aſcending to Sanha, and near to the river for ten miles.

The thermometer falling ſome degrees, we found it cold and chilly. The bed of the river is full of large ſtones, probably waſhed down from the mountains by the rapidity of its ſtream; they are chiefly quartz and granite. Here is excellent paſture for numerous herds of goats.

Road to Chichakumboo. From Sanha the aſcent is much greater, and, after keeping for ten miles along the banks of the Pachu, ſtill a conſiderable ſtream, we reach its ſource (from three diſtinct rivulets, all in view, ramified and ſupplied by numerous ſprings), and ſoon after arrive at the moſt elevated part of our road.

Here we quit the boundary of Boutan, and enter the territory of Thibet, where nature has drawn the line ſtill more ſtrongly, and affords, perhaps, the moſt extraordinary contraſt

traſt that takes place on the face of the earth. From this eminence are to be ſeen the mountains of Boutan, covered with trees, ſhrubs, and verdure to their tops, and on the ſouth ſide of this mountain to within a few feet of the ground on which we tread. On the north ſide the eye takes in an extenſive range of hills and plains, but not a tree, ſhrub, or ſcarce a tuft of graſs to be ſeen. Thus, in the courſe of leſs than a mile, we bid adieu to a moſt fertile ſoil, covered with perpetual verdure, and enter a country where the ſoil and climate ſeem inimical to the production of every vegetable. The change in the temperature of the air is equally obvious and rapid. The thermometer in the forenoon 34°, with froſt and ſnow in the night-time. Our preſent obſervations on the cauſe of this change confirmed us in a former opinion, and inconteſtably prove, that we are to look for that difference of climate from the ſituation of the ground as more or leſs above the general level of the earth. In attending to this cauſe of heat or cold, we muſt not allow ourſelves to be deceived by a compariſon with that which is immediately in view. We ought to take in a greater range of country, and where the road is near the banks of a river, we cannot well err in forming a judgement of the inclination of the ground. Punukha and Wandepore, both to the northward of Taſſeſudon, are quite in a Bengal climate. The thermometer at the firſt of theſe places, in the months of July and January, was within two degrees of what it had been at Rungpore for the ſame periods. They ſeem in more expoſed ſituations than Taſſeſudon; and, were we to draw a compariſon of their heights from the ſurrounding ground, I ſhould ſay they were above its level. The road, however, proves the reverſe. From Punukha to

Taſſeſudon we had a continued and ſteep aſcent for ſix hours and a half, with a very inconſiderable deſcent on the Taſſeſudon ſide. From the ſouth ſide of the mountain dividing Boutan from Thibet, the ſprings and rivulets are tumbling down in caſcades and torrents, and have been traced by us near to the foot of the hills, where they empty themſelves to the eaſtward of Buxaduar. On the north ſide they glide ſmoothly along, and by paſſing to the northward as far as Tiſhoolumboo, prove a deſcent on that ſide, which the eye could not detect. This part of the country, being the moſt elevated, is at all times the coldeſt; and the ſnowy mountains, from their heights and bearings, notwithſtanding the diſtance, are certainly thoſe ſeen from Purnea.

The ſoil on the Thibet ſide of the mountain is ſandy, with much gravel and many looſe ſtones. On the road found the Aconitum pyreneum, and two ſpecies of the Saxifraga.

See a large flock of chowry tailed cattle; their extenſive range of paſture ſeems to make amends for its poverty.

From Faro to Duina, Sept. 15. From Faro to Duina paſs over an extenſive plain, bounded by many ſmall hills, oddly arranged; ſome of them detached and ſingle, and all ſeem compoſed of ſand collected in that form, having the plain for their general baſe.

At Duina found a few plots of barley, which they are now cutting down, though green, as deſpairing of its ripening. The thermometer, at ſix o'clock in the morning, below the freezing point, and the ground partially covered with ſnow.

Road to Chalu, Sept. 16. Continue on the plain; find three ſprings forcing their way through the ground with violence, and giving riſe to a lake many miles in extent, ſtored

with millions of water-fowl and excellent fish. Of the first saw the cyrus, solan geese, many kinds of ducks, pintados, cranes, and gulls of different sorts. The springs of this lake are in great reputation for the cure of most diseases. I examined the water, and found it contain a portion of alum with the selenitic earth. On the banks of the lake I found a crystallization, which proves to be an alkaline salt; it is used by the natives for washing, and answers the purpose as well as pot-ash. The pasture which is impregnated with this salt is greedily sought after by sheep and goats, and proves excellent food for them. The hills are chiefly composed of sand incrusted by the inclemency of the weather and violent winds, seeming at first view composed of freestone.

Road to Simadar, Sept. 17. Pass a lake still more considerable than the former, with which it communicates by a narrow stream, about three miles long. There never was a more barren or unpromising soil; little turf, grass, or vegetation of any sort, except near the lake. See a few huts, mostly in ruins and deserted. The only grain in this part of the country is barley, which they are cutting down every where green.

Pass two springs, one of them slightly impregnated with alum. They form the principal source of a river, which empties itself in the Burrampooter near Tissoolumboo.

The wind from the eastward of south is now the coldest and most piercing; passing over the snowy mountains and dry sandy desart before described, it comes divested of all vapour or moisture, and produces the same effect as the hot dry winds in more southerly situations. Mahogany boxes and furniture, that had withstood the Bengal climate for years, were warped with

with conſiderable fiſſures, and rendered uſeleſs. The natives ſay, a direct expoſure to theſe winds occaſions the loſs of their fore-teeth; and our faithful guide aſcribed that defect in himſelf to this cauſe. We eſcaped with loſs of the ſkin from the greateſt part of our faces.

Road to Seluh, Sept. 18. Near our road to-day found a hot well, much frequented by people with venereal complaints, rheumatiſm, and all cutaneous diſeaſes. They do not drink the water, but uſe it as a bath. The thermometer, when immerſed in the water, roſe from 40° to 88°. It has a ſtrong ſulphureous ſmell, and contains a portion of hepar ſulphuris. Expoſure to air deprives it, as moſt other mineral wells, of much of its property.

Road to Takui, Sept. 19. Paſs ſome fields of barley and peaſe, and get into a milder climate. Find to-day a great variety of ſtone and rock, ſome containing copper, and others, a very pure rock cryſtal, regularly cryſtallized, with ſix unequal ſides. The rock cryſtal is of different ſizes and degrees of purity, but of one form. Find ſome flint and granite, ſeveral ſprings of water impregnated with iron, and nearly of the ſame temperature with the atmoſphere. See a few ill-thriving willows planted near the habitations, and which are the only trees to be met with.

Road to Tiſſoolumboo, Sept. 20, 21, and 22. The remaining part of our journey is over a more fertile ſoil, enjoying a milder climate. Some very good fields of wheat, barley, and peaſe; many pleaſant villages and diſtant houſes, leſs ſand and more rock, part flaty, and much of it a very good ſort of flint. The ſoil in the valley a light-coloured clay and ſand. They are every where employed in cutting down their crop. What

a happy climate! The ſky ſerene and clear, without a cloud; and ſo confident are they of the continuance of this weather, that their crop is thrown together in a convenient part of the field, without any cover, to remain till they can find time to threſh it out,

Before we reached Tiſſoolumboo found ſome elms and aſh-trees.

The hills in Thibet have, from their general appearance, ſtrong marks of containing thoſe foſſils that are inimical to vegation; ſuch are moſt of the ores of metal and pyritical matter.

The country, properly explored, promiſes better than any I have ſeen to gratify the curioſity of a philoſopher, and reward the labours of a mineraliſt. Accident, more than a ſpirit of enterpriſe and enquiry, has already diſcovered the preſence of many valuable ores and minerals in Thibet. The firſt in this liſt is deſervedly gold. They find it in large quantities, and frequently very pure. In the form of gold-duſt it is found in the beds of rivers, and at their ſeveral bendings, generally attached to ſmall pieces of ſtone, with every appearance of its having been part of a larger maſs. They find it ſometimes in large maſſes, lumps, and irregular veins; the adhering ſtone is generally flint, or quartz, and I have ſometimes ſeen a half-formed, impure ſort of precious ſtone in the maſs. By a common proceſs for the purification of gold, I extracted 12 *per cent.* of refuſe from ſome gold-duſt, and on examination found it to be ſand and filings of iron, which laſt was not likely to have been with it in its native ſtate, but probably employed for the purpoſe of adulteration. Two days journey from Tiſſoolumboo there is a lead mine. The ore is much the ſame as that found in Derbyſhire, mineralized by ſulphur,

ſulphur, and the metal obtained by the very ſimple operation of fuſion alone. Moſt lead contains a portion of ſilver, and ſome in the proportion to make it an object to work the lead ore for the ſake of the ſilver. Cinnabar, containing a large proportion of quickſilver, is found in Thibet, and might be advantageouſly employed for the purpoſe of extracting this metal. The proceſs is ſimple, by diſtillation; but to carry it on in the great would require more fuel than the country can well ſupply. I have ſeen ores and looſe ſtones containing copper, and have not a doubt of its being to be found in great abundance in the country. Iron is more frequently to be met with in Boutan than Thibet; and, was it more common, the difficulty of procuring proper fuel for ſmelting the leſs valuable ores, muſt prove an inſuperable objection to the working them. The dung of animals is the only ſubſtitute they have for fire-wood, and with that alone they will never be able to excite a degree of heat ſufficiently intenſe for ſuch purpoſes. Thus ſituated, the moſt valuable diſcovery for them would be that of a coal mine. In ſome parts of China bordering on Thibet, coal is found and uſed as fuel.

Tincal, the nature and production of which we have only hitherto been able to gueſs at, is now well known, and Thibet, from whence we are ſupplied, contains it in inexhauſtible quantities. It is a foſſil brought to market in the ſtate it is dug out of the lake, and afterwards refined into Borax by ourſelves. Rock ſalt is likewiſe found in great abundance in Thibet.

The lake, from whence tincal and rock ſalt are collected, is about fifteen days journey from Tiſſoolumboo, and to the northward of it. It is encompaſſed on all ſides by rocky hills, without

without any brooks or rivulets near at hand; but its waters are ſupplied by ſprings, which being ſaltiſh to the taſte are not uſed by the natives. The tincal is depoſited or formed in the bed of the lake; and thoſe who go to collect it, dig it up in large maſſes, which they afterwards break into ſmall pieces for the convenience of carriage, expoſing it to the air to dry. Although tincal has been collected from this lake for a great length of time, the quantity is not perceptibly diminiſhed; and as the cavities made by digging it ſoon wear out or fill up, it is an opinion with the people, that the formation of freſh tincal is going on. They have never yet met with it in dry ground or high ſituations, but it is found in the ſhalloweſt depths, and the borders of the lake, which deepening gradually from the edges towards the center contains too much water to admit of their ſearching for the tincal conveniently; but from the deepeſt parts they bring up rock ſalt, which is not to be found in the ſhallows, or near the bank. The waters of the lake riſe and fall very little, being ſupplied by a conſtant and unvarying ſource, neither augmented by the influx of any current, or diminiſhed by any ſtream running from it. The lake, I am aſſured, is at leaſt twenty miles in circumference, and ſtanding in a very bleak ſituation is frozen for a great part of the year. The people employed in collecting theſe ſalts are obliged to deſert from their labour ſo early as October, on account of the ice. Tincal is uſed in Thibet for ſoldering and to promote the fuſion of gold and ſilver. Rock ſalt is univerſally uſed for all domeſtic purpoſes in Thibet, Boutan, and Naphaul.

The thermometer at Tiſſoolumboo during the month of October was, on an average, at eight o'clock in the morning 38°, at noon 46°, and ſix o'clock in the evening 42°. The

weather clear, cool, and pleaſant, and the prevailing wind from the ſouthward. During the month of November we had froſts morning and evening, a ſerene clear ſky, not a cloud to be ſeen. The rays of the ſun paſſing through a medium ſo little obſcured had great influence. The thermometer was often below 30° in the morning, and ſeldom above 38° at noon in the ſhade; wind from the ſouthward.

Of the diſeaſes of this country, the firſt that attracts our notice, as we approach the foot of the hills, is a glandular ſwelling in the throat, which is known to prevail in ſimilar ſituations in ſome parts of Europe, and generally aſcribed to an impregnation of the water from ſnow. The diſeaſe being common at the foot of the Alps, and confined to a tract of country near theſe mountains, has firſt given riſe to the idea of its being occaſioned by ſnow water. If a general view of the diſeaſe, and ſituations where it is common, had been the ſubject of enquiry, or awakened the attention of any able practitioner, we ſhould have been long ſince undeceived in this reſpect. On the coaſt of Greenland, the mountainous parts of Wales and Scotland, where melted ſnow muſt be continually paſſing into their rivers and ſtreams, the diſeaſe is not known, though it is common in Derbyſhire, and ſome other parts of England. Rungpore is about one hundred miles from the foot of the hills, and much farther from the ſnow, yet the diſeaſe is as frequent there as in Boutan. In Thibet, where ſnow is never out of view, and the principal ſource of all their rivers and ſtreams, the diſeaſe is not to be met with; but what puts the matter paſt a doubt, is the frequency of the diſeaſe on the coaſt of Sumatra, where ſnow is never to be found. On finding the vegetable productions of Boutan the ſame as thoſe of the Alps in almoſt every inſtance, it occurred to me, that the

the diſeaſe might ariſe from an impregnation of the water by theſe plants, or the ſoil probably poſſeſſing ſimilar qualities, the ſpontaneous productions of both countries, with very few exceptions, being ſo nearly alike. It however appears more probable, that the diſeaſe is endemial, proceeding from a peculiarity in the air of ſituations in the vicinity of mountains with ſuch ſoil and vegetable productions. I am the more inclined to think ſo, that I have univerſally found this diſeaſe moſt prevalent amongſt the lower claſs of people, and thoſe who are moſt expoſed to the unguarded influence of the weather, and various changes that take place in the air of ſuch ſituations. The primary cauſe in the atmoſphere producing this effect is, perhaps, not more inexplicable than what we meet with in the low-lands of Eſſex and fens of Lincolnſhire. An accurate analyſis of the water uſed in common by the natives, where this diſeaſe is more or leſs frequent, and where it is not known in ſimilar expoſures, might throw ſome light on this ſubject.

This very extraordinary diſeaſe has been little attended to, from obvious reaſons; it is unaccompanied with pain, ſeldom fatal, and generally confined to the poorer ſort of people. The tumor is unſightly, and grows to a troubleſome ſize, being often as large as a perſon's head. It is certainly not exaggerating to ſay, that one in ſix of the Rungpore diſtrict, and country of Boutan, has the diſeaſe.

As thoſe who labour moſt, and are the leaſt protected from the changes of weather, are moſt ſubject to the diſeaſe, we univerſally find it in Boutan more common with the women than men. It generally appears in Boutan at the age of thirteen or fourteen, and in Bengal at the age of eleven or twelve; ſo that in both countries the diſeaſe ſhows itſelf about the age

of puberty. I do not believe this diſeaſe has ever been removed, though a mercurial courſe ſeemed to check its progreſs, but did not prevent its advance after intermitting the uſe of mercury. An attention to the primary cauſe will firſt lead to a proper method of treating the diſeaſe; a change of ſituation for a ſhort while, at that particular period when it appears, might be the means of preventing it.

The people of this happy climate are not exempt from the venereal diſeaſe, which ſeems to rage with unremitting fury in all climates, and proves the greateſt ſcourge to the human race. It has been long a matter of doubt, whether this diſeaſe has ever been cured by any other ſpecific than mercury and its different preparations. In defence of the opinion of other ſpecifics being in uſe, it has always been urged, that the diſeaſe is frequent in many parts of the world, where it could not be ſuppoſed that they were acquainted with quickſilver, and the proper method of preparing it as a medicine. I muſt own, that I expected to have been able to have added one other ſpecific for this diſeaſe to our liſt in the Materia medica, being informed that the diſeaſe was common, and their method of treating it ſucceſsful; nor could I allow myſelf to think they were acquainted with the method of preparing quickſilver, ſo as to render it a ſafe and efficacious medicine. In this, however, I was miſtaken.

The diſeaſe ſeems in this country to make a more rapid progreſs, and rage with more violence, than in any other. This is to be accounted for from the groſsneſs of their food and little attention to cleanlineſs.

There is one preparation of mercury in common uſe with them, and made after the following manner. A portion of alum, nitre, vermillion, and quickſilver, are placed in the bottom of an earthen

earthen pot, with a ſmaller one inverted put over the materials, and well luted to the bottom of the larger pot. Over the ſmall one, and within the large one, the fuel is placed, and the fire continued for about forty minutes. A certain quantity of fuel, carefully weighed out, is what regulates them with reſpect to the degree of heat, as they cannot ſee the materials during the operation. When the veſſel is cool, the ſmall inverted pot is taken off, and the materials collected for uſe. I attended the whole of the proceſs, and examined the materials afterwards. The quickſilver had been acted on by the other ingredients, deprived of its metallic form, and rendered a ſafe and efficacious remedy.

A knowledge of chemiſtry has taught us a more certain method of rendering this valuable medicine active and efficacious; yet we find this preparation anſwering every good purpoſe, and by their guarded manner of exhibiting it perfectly ſafe. This powder is the baſis of their pill, and often uſed in external application. The whole, when intimately mixed, formed a reddiſh powder, and was made into the form of pills by the addition of a plum or date. Two or three pills taken twice a day generally bring on, about the fourth or fifth day, a ſpitting, which is encouraged by continuing the uſe of the pills for a day or two longer. As the ſalivation advances, they put a ſtick acroſs the patient's mouth, in the form of a gag, and make it faſt behind. This, they ſay, is done to promote the ſpitting, and prevent the loſs of their teeth. They keep up the ſalivation for ten or twelve days, during which time the patient is nouriſhed with congee and other liquids. Part of this powder is often uſed externally by diffuſing it in warm water, and waſhing ſores and buboes. They diſperſe buboes frequently by poultices of turnip tops, in which they always

put

put vermillion, and ſometimes muſk. Nitre, as a cooler, is very much uſed internally by them in this diſeaſe, and they ſtrictly enjoin warmth and confinement during the ſlighteſt mercurial courſe. Buboes advanced to ſuppuration are opened by a lancet, with a large inciſion, which they do not allow to cloſe before the hardneſs and tumor are gone. In ſhort, I found very little room for improving their practice in this diſeaſe. I introduced the method of killing quickſilver with honey, gave them an opportunity of ſeeing it done, and had the ſatisfaction of finding it ſucceſsfully uſed by themſelves before we left the country.

This happy climate preſents us with but little variety in their diſeaſes. Coughs, colds, and rheumatiſm, are more frequent here than in Bengal. Fevers generally ariſe here from a temporary cauſe, are eaſily removed, and ſeldom prove fatal. The liver diſeaſe is occaſionally to be met with, and complaints in the bowels are not unfrequent; but the groſſneſs of their food, and uncleanlineſs of their perſons, would in any other climate be the ſource of conſtant diſeaſe and ſickneſs. They are ignorant (as we were, not many years ago) of the proper method of treating diſeaſes of the liver and other viſcera; this is, I believe, the cauſe of the moſt obſtinate and fatal diſeaſe to be met with in the country, I mean the dropſy. As the Rajah had ever been deſirous of my aid and advice, and had directed his doctors to attend to my private inſtructions and practice, I endeavoured to introduce a more judicious method of treating thoſe diſeaſes by mercurial preparations. I had an opportunity of proving the advantage of this plan to their conviction in ſeveral inſtances, and of ſeeing them initiated in the practice.

The Rajah favoured me with above ſeventy ſpecimens of the medicines in uſe with them. They have many ſorts of ſtones and petrifactions

petrifactions ſaponaceous to the touch, which are employed as an external application in ſwellings and pains of the joints. They often remove ſuch complaints, and violent head-achs, by fumigating the part affected with aromatic plants and flowers. They do not ſeek for any other means of information reſpecting the ſtate of a patient than that of feeling the pulſe; and they confidently ſay, that the ſeat of pain and diſeaſe is eaſily to be diſcovered, not ſo much from the frequency of the pulſe as its vibratory motion. They feel the pulſe at the wriſt with their three fore-fingers, firſt of the right, and then of the left hand; after preſſing more or leſs on the artery, and occaſionally removing one or two of the fingers, they determine what the diſeaſe is. They do not eat any thing the day on which they take phyſic, but endeavour to make up the loſs afterwards by eating more freely than before, and uſing ſuch medicines as they think will occaſion coſtiveneſs.

The many ſimples in uſe with them are from the vegetable kingdom, collected chiefly in Boutan. They are in general inoffenſive and very mild in their operation. Carminatives and aromatics are given in coughs, colds, and affections of the breaſt. The centaury, coriander, carraway, and cinnamon, are of this ſort. This laſt is with them the bark of the root of that ſpecies of Laurus formerly mentioned as a native of this country. The bark from the root is in this plant the only part which partakes of the cinnamon taſte; and I doubt very much if it could be diſtinguiſhed by the beſt judges from what we call the true cinnamon. The bark, leaves, berries, and ſtalks of many ſhrubs and trees, are in uſe with them, all in decoction. Some have much of the aſtringent bitter taſte of our moſt valuable medicines, and are generally employed here with the ſame view, to ſtrengthen the powers of digeſtion, and mend

the general habit. Their principal purgative medicines are brought by the Chineſe to Laſſa. They had not any medicine that operated as a vomit, till I gave the Rajah ſome ipecacuanha, who made the firſt experiment with it on himſelf.

In bleeding they have a great opinion of drawing the blood from a particular part. For head-achs they bleed in the neck; for pains in the arm and ſhoulder, in the cephalic vein; and of the breaſt or ſide, in the median; and if in the belly, they bleed in the baſilic vein. They think pains of the lower extremity are beſt removed by bleeding in the ancle. They have a great prejudice againſt bleeding in cold weather; nor is any urgency or violent ſymptom thought at that time a ſufficient reaſon for doing it.

They have their lucky and unlucky days for operating or taking any medicine; but I have known them get the better of this prejudice, and be prevailed on.

Cupping is much practiſed by them; a horn, about the ſize of a cupping glaſs, is applied to the part, and by a ſmall aperture at the other end they extract the air with their mouth. The part is afterwards ſcarified with a lancet. This is often done on the back; and in pain and ſwelling of the knee it is held as a ſovereign remedy. I have often admired their dexterity in operating with bad inſtruments. Mr. HAMILTON gave them ſome lancets, and they have ſince endeavoured, with ſome ſucceſs, to make them of that form They were very thankful for the few I could ſpare them. In fevers they uſe the Kuthuilega nut, well known in Bengal as an efficacious medicine. They endeavour to cure the dropſy by external applications, and giving a compounded medicine made up of above thirty different ingredients: they ſeldom or never ſucceed in effecting a cure of this diſeaſe.

I explained to the Rajah the operation of tapping, and ſhewed him the inſtrument with which it was done. He very earneſtly expreſſed a deſire that I ſhould perform the operation, and wiſhed much for a proper ſubject; ſuch a one did not occur while I remained, and perhaps it was as well both for the Rajah's patients and my own credit; for after having ſeen it once done, he would not have heſitated about a repetition of the operation. Graveliſh complaints and the ſtone in the bladder are, I believe, diſeaſes unknown here.

The ſmall-pox, when it appears among them, is a diſeaſe that ſtrikes them with too much terror and conſternation to admit of their treating it properly. Their attention is not employed in ſaving the lives of the infected, but in preſerving themſelves from the diſeaſe. All communication with the infected is ſtrictly forbidden, even at the riſk of their being ſtarved, and the houſe or village is afterwards eraſed. A promiſcuous and free intercourſe with their neighbours not being allowed, the diſeaſe is very ſeldom to be met with, and its progreſs always checked by the vigilance and terror of the natives. Few in the country have had the diſeaſe. Inoculation, if ever introduced, muſt be very general to prevent the devaſtation that would be made by the infection in the natural way; and where there could not be any choice in the ſubject fit to receive the diſeaſe, many muſt fall a ſacrifice to it. The preſent Rajah of Thibet was inoculated, with ſome of his followers, when in China with the late Tiſhoo Lama.

The hot bath is uſed in many diſorders, particularly in complaints of the bowels and cutaneous eruptions. The hot wells of Thibet are reſorted to by thouſands. In Boutan they ſubſtitute water warmed by hot ſtones thrown into it.

In Thibet the natives are more ſubject to ſore eyes and blindneſs than in Boutan. The high winds, ſandy ſoil, and glare from the reflection of the ſun, both from the ſnow and ſand, account for this.

I have dwelt long on this ſubject, becauſe I think the knowledge and obſervations of theſe people on the diſeaſes of their country, with their medical practice, keep pace with a refinement and ſtate of civilization, which ſtruck me with wonder, and no doubt will give riſe to much curious ſpeculation, when known to be the manners of a people holding ſo little intercourſe with what we term civilized nations.

Dec. 1. Left Tiſhoolumbo, and found the cold increaſe every day as we advanced to the ſouthward, moſt of the running waters frozen, and the pools covered with ice ſtrong enough to carry. Our thermometer having only the ſcale as low as 16°, we could not preciſely determine the degree of cold, the quickſilver being under that every morning. The froſt is certainly never ſo intenſe in Great-Britain. On our return to the lakes the 14th, we found them deſerted by the water fowl, and were informed that they had been one ſolid piece of ice ſince the 10th of November. Here we reſumed our amuſement of ſkating, to the great aſtoniſhment of the natives and Bengal ſervants.

On the 17th we re-entered Boutan, and in ſix days more arrived at Punukha by Paraghou. No ſnow or froſt to be met with in Boutan, except towards the tops of their higheſt mountains; the thermometer riſing to 36° in the morning, and 48° at noon.

Took leave of the Debe Rajah, and on the 12th arrived at Buxaduar.

Calcutta, Feb. 17, 1784.

AS Lac is the produce of, and a ſtaple article of commerce in Aſſam, a country bordering on and much connected with Thibet, ſome account of it may not be an improper ſupplement to the above remarks.

Lac is, ſtrictly ſpeaking, neither a gummy nor reſinous ſubſtance, though it has ſome properties in common to both. Gums are ſoluble in water, and reſins in ſpirits; lac admits of a very difficult union with either, without the mediation of ſome other agent.

Lac is known in Europe by the different appellations of ſtick lac, ſeed lac, and ſhell lac. The firſt is the lac in pretty conſiderable lumps, with much of the woody parts of the branches on which it is formed adhering to it. Seed lac is only the ſtick lac broke into ſmall pieces, garbled, and appearing in a granulated form. Shell lac is the purified lac, by a very ſimple proceſs to be mentioned afterward.

Many vague and unauthenticated reports concerning lac have reached the public; and though amongſt the multiplicity of accounts the true hiſtory of this ſubſtance has been nearly hit on, little credit is given in Europe to any deſcription of it hitherto publiſhed. My obſervations, as far as they go, are the reſult of what I have ſeen, from the lac on the tree, the progreſs of the inſect now in my cuſtody, and the information of a gentleman reſiding at Goalpara on the borders of Aſſam, who is perfectly verſant in the method of breeding the inſect, inviting it to the tree, collecting the lac from the branches, and forming it into ſhell lac, in which ſtate much of it is received from Aſſam, and exported to Europe for various great and uſeful purpoſes. The tree on which this fly moſt commonly generates is known in Bengal by the name of the *Biher* tree, and is a ſpecies of

the Rhamnus. The fly is nouriſhed by the tree, and there depoſits its eggs, which nature has provided it with the means of defending from external injury by a collection of this lac, evidently ſerving the twofold purpoſe of a nidus and covering to the ovum and inſect in its firſt ſtage, and food for the maggot in its more advanced ſtate. The lac is formed into complete cells, finiſhed with as much regularity and art as a honey-comb, but differently arranged. The flies are invited to depoſit their eggs on the branches of the tree, by beſmearing them with ſome of the freſh lac ſteeped in water, which attracts the fly, and gives a better and larger crop.

The lac is collected twice a year, in the months of February and Auguſt.

I have examined the egg of the fly with a very good microſcope; it is of a very pure red, perfectly tranſparent, except in the centre, where there were evident marks of the embryo forming, and opaque ramifications paſſing off from the body of it. The egg is perfectly oval, and about the ſize of an ant's egg. The maggot is about the one-eighth of an inch long, formed of many rings (ten or twelve) with a ſmall red head; when ſeen with a microſcope, the parts of the head were eaſily diſtinguiſhed, with ſix ſmall ſpecks on the breaſt, ſomewhat projecting, which ſeemed to be the incipient formation of the feet. This maggot is now in my cuſtody, in the form of a nymph or cryſalis, its annular coat forming a ſtrong covering, from which it ſhould iſſue forth a fly. I have never ſeen the fly, and cannot therefore deſcribe it more fully, or determine its genus and ſpecies. I am promiſed a drawing of the inſect in its different ſtages, and ſhall be able ſoon to add to a botanical deſcription of the plant a drawing of the branch, with the different parts of fructification and lac on it.

The gentleman to whom I owe part of my information terms the lac the excrement of the inſect. On a more minute inveſtigation, however, we may not find it more ſo than the wax or honey of the bee, or ſilk of the ſilk-worm. Nature has provided moſt inſects with the means of ſecreting a ſubſtance which generally anſwers the twofold purpoſe of defending the embryo, and ſupplying nouriſhment to the inſect from the time of its animation till able to wander abroad in queſt of food. The freſh lac contains within its cells a liquid, ſweetiſh to the taſte, and of a fine red colour, miſcible in water. The natives of Aſſam uſe it as a dye, and cotton dipped in this liquid makes afterwards a very good red ink.

The ſimple operation of purifying lac is practiſed as follows. It is broken into ſmall pieces, and picked from the branches and ſticks, when it is put into a ſort of canvas bag of about four feet long, and not above ſix inches in circumference. Two of theſe bags are in conſtant uſe, and each of them held by two men. The bag is placed over a fire, and frequently turned till the lac is liquid enough to paſs through its pores, when it is taken off the fire, and ſqueezed by two men in different directions, dragging it along the convex part of a plantain-tree prepared for the purpoſe; while this is doing, the other bag is heating, to be treated in the ſame way. The mucilaginous and ſmooth ſurface of the plantain-tree ſeems peculiarly well adapted for preventing the adheſion of the heated lac, and giving it the form which enhances its value ſo much. The degree of preſſure on the plantain-tree regulates the thickneſs of the ſhell, and the quality of the bag determines its fineneſs and tranſparency. They have learned of late, that the lac which is thicker in the ſhell than it uſed to be, is moſt prized in Europe. Aſſam furniſhes us with the greateſt quantity

tity of lac in uſe; and it may not be generally known, that the tree on which they produce the beſt and largeſt quantity of lac is not uncommon in Bengal, and might be employed in propagating the fly, and cultivating the lac, to great advantage. The ſmall quantity of lac collected in theſe provinces affords a precarious and uncertain crop, becauſe not attended to. Some attention at particular ſeaſons is neceſſary to invite the fly to the tree; and collecting the whole of the lac with too great an avidity, where the inſect is not very generally to be met with, may annihilate the breed.

The beſt method of cultivating the tree, and preſerving the inſect, being properly underſtood in Bengal, would ſecure to the Coſs poſſeſſions the benefit ariſing from the ſale of a lucrative article, in great demand and of extenſive uſe.

Stages and diſtance from Rungpore to Taſſeſudon and Tiſſoolumboo, in computed coſſes and miles, two miles to a coſs.

		Coſſes.	M.	F.
1783,	From Rungpore to			
May 6.	Calamaty Plains	5½	11	0
8.	To Mongulhaut	5½	11	4
9.	To Belladinga	7	14	0
10.	To Bahar	4	8	0
11.	To Chichacotta in Boutan	13	26	0
12.	To Buxaduar	12	24	0
22.	To Joogagoo	5	10	0
23.	To Muriſhong	5	10	0
25.	To Chooka	9	18	0
		66	132	4

		Coffes.	M. F.
	Brought over	66	132 4
1783, May 26.	To Punuka - -	7	14 0
27.	To Chepta - -	5	10 0
29.	To Pagha - -	5	10 0
30.	To Numloo - -	4	8 0
31.	To Wanakha - -	4	8 0
June 1.	To Taſſeſudon, capital of Boutan	3	6 0
		94	188 4
Sept. 8.	To Pimitung - -	7	14 0
9.	To Paraghon - -	6	12 0
11.	To Dukaigun - -	4	8 0
12.	To Sanha - -	5	10 0
13.	To a tent on Thibet ground -	8	16 0
14.	To Chichakumboo, Thibet -	4	8 0
15.	To Duina - -	10	20 0
16.	To Chalu - -	15	30 0
17.	To Simadar - -	9	18 0
18.	To Selu - - -	17	34 0
19.	To Takui - -	9	18 0
20.	To Dequini - -	14	28 0
21.	To Sehundi - -	15	30 0
22.	To Tiſſoolumboo, capital of Thibet	7	14 0
		224	448 4

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METEOROLOGICAL JOURNAL

KEPT AT THE APARTMENTS OF

THE ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January 1788.

1788	Time.	Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Jan. 1	8 0	43	49	29,97		SSE	1	Cloudy.
	2 0	46	51	29,91		S	1	Fine.
2	8 0	48	49	29,58	0,040	S	2	Cloudy.
	2 0	48	52	29,24		S	2	Rainy.
3	8 0	42	50	28,97	0,235	S	2	Fair.
	2 0	46	52	28,89		S	2	Rainy.
4	8 0	43	51	29,13	0,024	SW by W	1	Fair.
	2 0	44	56	29,23		WSW	1	Fine.
5	8 0	37	52	29,35		SW	1	Fine.
	2 0	40	56	29,32		SW	1	Fine.
6	8 0	42	53	29,16		SW	1	Cloudy.
	2 0	42	56	29,37		SW	1	Cloudy.
7	8 0	37	52	29,66		WSW	1	Cloudy.
	2 0	41	54	29,78		W	1	Cloudy.
8	8 0	39	52	29,96		ENE	1	Cloudy.
	2 0	43	53	30,01		ENE	1	Cloudy.
9	8 0	40	51	30,18	0,020	E	2	Rainy.
	2 0	40	52	30,17		E	2	Cloudy.
10	8 0	37	51	30,21		ENE	1	Cloudy.
	2 0	40	53	30,21		NE	1	Cloudy.
11	8 0	37	51	30,32		NE	1	Cloudy.
	2 0	39	53	30,36		NE	1	Cloudy.
12	8 0	36	52	30,38		W	1	Foggy.
	2 0	39	53	30,36		WNW	1	Fine.
13	8 0	39	52	30,21		WNW	1	Cloudy.
	2 0	41	53	30,11		WNW	1	Cloudy.
14	8 0	36	52	29,98		W	1	Fine.
	2 0	41	53	30,19		W	2	Cloudy.
15	8 0	27	50	30,48		WNW	1	Fine.
	2 0	33	52	30,56		WNW	1	Fine.
16	8 0	26	50	30,68		WNW	1	Fine.
	2 0	36	52	30,70		WNW	1	Fine.

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for January 1788.

1788	Time. H.	Time. M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Jan. 17	8	0	33	50	30,66		W	1	Foggy.
	2	0	40	53	30,63		W	1	Cloudy.
18	8	0	36	50	30,23		W	1	Cloudy.
	2	0	45	52	29,98		W	1	Cloudy.
19	8	0	37	50	29,68	0,060	WSW	2	Cloudy.
	2	0	43	53	29,85		WSW	2	Cloudy.
20	8	0	34	51	30,33		NW	1	Cloudy.
	2	0	40	53	30,38		WNW	1	Cloudy.
21	8	0	39	52	30,33		WNW	1	Cloudy.
	2	0	44	54	30,24		WSW	1	Cloudy.
22	8	0	37	52	29,97		W	1	Fine.
	2	0	44	54	30,00		WSW	2	Cloudy.
23	8	0	39	52	30,15		WNW	2	Cloudy.
	2	0	44	54	30,07		WNW	2	Cloudy.
24	8	0	44	52	29,86		W	2	Cloudy.
	2	0	48	54	29,84		W	2	Cloudy.
25	8	0	43	54	29,90	0,020	W	1	Cloudy.
	2	0	45	56	30,02		W	1	Fine.
26	8	0	39	55	30,06		W	1	Fine.
	2	0	46	55	29,98		WSW	1	Cloudy.
27	8	0	42	54	29,97	0,040	S by W	1	Cloudy.
	2	0	45	56	30,02		S	1	Cloudy.
28	8	0	38	54	30,27		SW	1	Cloudy.
	2	0	39	56	30,31		WSW	1	Cloudy.
29	8	0	32	53	30,33		WNW	1	Foggy.
	2	0	35	53	30,35		N	1	Foggy.
30	8	0	33	52	30,38		NE	1	Cloudy.
	2	0	38	53	30,41		NE	1	Cloudy.
31	8	0	36	51	30,30		E	1	Cloudy.
	2	0	38	54	30,21		ESE	1	Cloudy.

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for February 1788.

1788	Time.	Therm. without	Therm within.	Barom.	Rain.	Winds.		Weather.
	H. M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Feb. 1	8 0	32	50	30,00		ESE	1	Cloudy.
	2 0	33	50	29,87		ESE	1	Cloudy.
2	8 0	29	50	29,66		N	1	Cloudy.
	2 0	37	51	29,55		NW	2	Cloudy.
3	8 0	43	50	29,26	0,076	WSW	2	Cloudy.
	2 0	46	52	29,40		WSW	2	Cloudy.
4	8 0	35	50	29,73		SW	1	Foggy.
	2 0	42	53	29,77		SW	1	Fine.
5	8 0	40	50	29,70	0,112	E	1	Cloudy.
	2 0	46	52	29,71		E	1	Cloudy.
6	8 0	35	51	30,15		W	1	Foggy.
	2 0	45	53	30,18		W	1	Cloudy.
7	8 0	37	52	30,21		NE	1	Foggy.
	2 0	46	54	30,21		NE	1	Cloudy.
8	8 0	39	53	30,13		NE	1	Cloudy.
	2 0	38	53	29,98		NE	1	Rainy.
9	8 0	33	51	29,80	0,067	NNE	1	Cloudy.
	2 0	33	53	29,73		NNE	1	Cloudy.
10	8 0	33	51	29,73		N	1	Cloudy.
	2 0	35	51	29,77		NW	1	Cloudy.
11	8 0	38	50	29,89		SE	1	Cloudy.
	2 0	45	54	29,98		SSW	1	Cloudy.
12	8 0	40	51	30,21		SW	1	Foggy.
	2 0	48	52	30,16		SW	1	Cloudy.
13	8 0	43	52	29,94	0,040	SW	1	Small rain.
	2 0	49	54	29,97		W	1	Fair.
14	8 0	40	52	30,12		W	1	Cloudy.
	2 0	49	54	30,06		W	1	Cloudy.
15	8 0	44	53	29,86	0,146	WNW	1	Cloudy.
	2 0	50	50	29,85		NW	1	Fine.
16	8 0	41	54	29,76	0,053	WSW	1	Rain.
	2 0	47	56	29,65		WSW	1	Fair.

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for February 1788.

1788	Time. H.	M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Feb. 17	8	o	46	56	29,71		W	1	Cloudy.
	2	o	47	56	29,75		SSW	1	Cloudy.
18	8	o	33	53	29,83	0,132	E	1	Cloudy.
	2	o	39	53	29,79		ESE	1	Fair.
19	8	o	39	52	29,45		E	1	Cloudy.
	2	o	44	54	29,33		SSE	1	Fine.
20	8	o	39	51	29,02	0,032	E	1	Cloudy.
	2	o	42	54	28,91		E	1	Cloudy.
21	8	o	41	52	28,71	0,235	E	1	Rainy.
	2	o	47	54	28,65		SSE	1	Rainy.
22	8	o	44	53	28,76	0,325	SW	1	Cloudy.
	2	o	47	54	28,94		WSW	1	Cloudy.
23	8	o	41	53	29,05		SW	1	Fine.
	2	o	48	54	29,08		SSW	1	Fair.
24	8	o	42	53	28,98		ESE	1	Cloudy.
	2	o	44	54	28,91		NE	1	Rain.
25	8	o	39	53	29,33	0,168	NW	1	Cloudy.
	2	o	43	54	29,50		WNW	1	Cloudy.
26	8	o	30	52	29,62		WNW	1	Fine.
	2	o	45	54	29,56		W	1	Fair.
27	8	o	41	52	29,31	0,038	E	1	Rain.
	2	o	44	54	29,35		E	1	Cloudy.
28	8	o	41	52	29,50		E	1	Cloudy.
	2	o	44	53	29,47		E	1	Cloudy.
29	8	o	42	53	29,28	0,037	SE	1	Cloudy.
	2	o	48	54	29,20		SSE	1	Cloudy.

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for March 1788.

1788	Time. H.	Time. M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Mar. 1	7	0	35	53	29,43		SW by S	1	Fine.
	2	0	45	54	29,48		SSW	1	Fair.
2	7	0	41	52	29,52		E	1	Cloudy.
	2	0	42	53	29,63		ESE	2	Cloudy.
3	7	0	35	51	29,99		E	2	Cloudy.
	2	0	41	53	30,08		E	2	Fair.
4	7	0	36	51	30,03		NW	1	Foggy.
	2	0	42	53	29,92		W	1	Fair.
5	7	0	33	51	29,97		NNW	1	Cloudy.
	2	0	41	53	29,88		W	1	Fair.
6	7	0	39	51	29,34	0,075	WSW	1	Fair.
	2	0	43	53	29,45		WNW	2	Cloudy.
7	7	0	34	50	29,43		NW	1	Fair.
	2	0	40	51	29,51		NW	1	Fair.
8	7	0	28	48	29,52		NE	1	Fair.
	2	0	39	50	29,54		NE	1	Fair.
9	7	0	29	48	29,59		NE	2	Fair.
	2	0	39	49	29,64		ENE	2	Fair.
10	7	0	30	47	29,89		ENE	1	Fine.
	2	0	38	50	29,97		ENE	1	Fair.
11	7	0	28	46	30,06		ENE	1	Fine.
	2	0	38	50	30,08		ENE	1	Fine.
12	7	0	29	47	30,03		E	1	Fine.
	2	0	39	51	30,00		ENE	1	Fine.
13	7	0	29	47	29,84		ESE	1	Cloudy.
	2	0	32	48	29,73		ESE	1	Cloudy.
14	7	0	31	46	29,52		E	1	Fine.
	2	0	39	49	29,44		ESE	1	Fair.
15	7	0	36	46	29,44		ESE	1	Cloudy.
	2	0	40	49	29,46		ESE	1	Cloudy.
16	7	0	38	47	29,46	0,120	ESE	1	Cloudy.
	2	0	37	47	29,46		ESE	2	Cloudy.

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for March 1788.

1788	Time. H.	M.	Therm without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Mar. 17	7	0	33	46	29,50		ESE	2	Cloudy.
	2	0	33	47	29,52		ESE	2	Cloudy.
18	7	0	33	46	29,68		ESE	2	Cloudy.
	2	0	39	49	29,75		ESE	2	Cloudy.
19	7	0	36	47	30,00		SE by E	1	Fair.
	2	0	42	49	30,03		SE by E	1	Fair.
20	7	0	41	47	29,82		SE	1	Cloudy.
	2	0	50	50	29,74		SW	1	Fair.
21	7	0	44	49	29,76	0,070	SSW	1	Fair.
	2	0	52	53	29,72		SW	1	Fair.
22	7	0	43	50	29,57		SSW	1	Cloudy.
	2	0	47	53	29,48		SSE	1	Cloudy.
23	7	0	39	50	29,32	0,071	N	2	Cloudy.
	2	0	44	53	29,47		N	2	Cloudy.
24	7	0	40	49	29,61		SW	2	Fair.
	2	0	51	53	29,59		SSW	2	Hazy.
25	7	0	46	51	29,52		SSW	2	Cloudy.
	2	0	52	54	29,58		SSW	2	Fair.
26	7	0	40	52	29,60		SSE	2	Fine.
	2	0	53	55	29,52		SSE	2	Fine.
27	7	0	42	54	29,52		S	1	Fine.
	2	0	51	57	29,44		SSE	2	Fine.
28	7	0	46	55	29,45		SSW	2	Fair.
	2	0	56	57	29,57		SW	2	Fair.
29	7	0	46	55	29,92		WSW	1	Cloudy.
	2	0	56	58	30,01		W	1	Fair.
30	7	0	50	56	29,95		WSW	1	Cloudy.
	2	0	59	58	29,90		SW	2	Fine.
31	7	0	48	58	29,63		W	2	Fine.
	2	0	49	59	29,68		W	2	Fair.

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for April 1788.

1788	Time. H.	M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
April 1	7	0	47	56	29,72	0,390	W	2	Rainy.
	2	0	55	60	29,74		WNW	2	Fair.
2	7	0	46	57	30,09		W	2	Fair.
	2	0	58	60	30,02		W	2	Fair.
3	7	0	47	58	29,72		W	2	Fair.
	2	0	54	60	29,50		W	2	Fair.
4	7	0	43	57	29,67		NW	2	Cloudy.
	2	0	40	58	29,75		NW by N	2	Hail.
5	7	0	40	55	29,97		NNW	2	Cloudy.
	2	0	41	57	30,10		N	2	Cloudy.
6	7	0	42	55	30,22	0,020	W	1	Fair.
	2	0	51	56	30,23		W	1	Cloudy.
7	7	0	48	55	30,27		W	1	Cloudy.
	2	0	51	56	30,30		WNW	1	Cloudy.
8	7	0	51	56	30,39		WNW	1	Cloudy.
	2	0	59	59	30,42		WNW	1	Cloudy.
9	7	0	48	56	30,48		WNW	1	Fair.
	2	0	59	59	30,47		NNW	1	Cloudy.
10	7	0	51	57	30,40		E	1	Cloudy.
	2	0	59	59	30,30		ESE	1	Fine.
11	7	0	49	57	30,16		E	1	Fine.
	2	0	58	59	30,07		SE	1	Fine.
12	7	0	48	57	29.97		SSW	1	Fine.
	2	0	56	58	30,09		SSW	1	Fine.
13	7	0	45	57	30,28		SW	1	Fine.
	2	0	59	61	30,25		SW by W	1	Fine.
14	7	0	45	58	30,06		W	1	Fine.
	2	0	53	61	30,00		W	1	Rainy.
15	7	0	43	57	30,03	0,097	W	1	Fine.
	2	0	51	59	30 06		W	1	Fair.
16	7	0	45	57	30,11		W	1	Fair.
	2	0	53	59	30,11		W	1	Fair.

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for April 1788.

1788	Time. H.	Time. M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Apr. 17	7	0	48	58	30,09		W	1	Cloudy.
	2	0	55	60	30,09		W	1	Cloudy.
18	7	0	50	59	30,21		W	1	Fair.
	2	0	62	61	30,26		N	1	Fair.
19	7	0	54	60	30,27		E	1	Fair.
	2	0	65	61	30,24		SE	1	Fine.
20	7	0	52	60	30,03		SSE	1	Fine.
	2	0	67	61	29,88		SSW	1	Fine.
21	7	0	45	59	29,76	0,100	SW	1	Fine.
	2	0	56	60	29,70		WSW	2	Fine.
22	7	0	46	59	29,95		SW	2	Fine.
	2	0	55	61	29,90		SW	1	Cloudy.
23	7	0	53	60	29,91		WSW	1	Cloudy.
	2	0	63	61	29,89		W	1	Cloudy.
24	7	0	47	60	29,95		W	1	Fine.
	2	0	56	60	30,01		WNW	2	Fine.
25	7	0	49	58	29,97		WNW	2	Fair.
	2	0	57	61	29,96		WNW	2	Fair.
26	7	0	54	59	30,15		W	1	Cloudy.
	2	0	61	60	30,18		W by S	2	Fair.
27	7	0	48	58	30,28		SW	1	Fine.
	2	0	63	61	30,28		WSW	1	Fine.
28	7	0	47	59	30,32		W	1	Fine.
	2	0	66	60	30,28		SE	1	Fine.
29	7	0	55	60	30,27		ESE	1	Fine.
	2	0	68	63	30,22		ESE	1	Fine.
30	7	0	55	60	30,22		ESE	1	Fine.
	2	0	68	63	30,20		ESE	1	Fine.

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for May 1788.

1788	Time. H.	M.	Therm without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
May 1	7	0	54	66	30,18		ESE	1	Fine.
	2	0	69	68	30,16		ESE	1	Fine.
2	7	0	56	61	30,14		NE	1	Fine.
	2	0	71	64	30,14		E	2	Fine.
3	7	0	50	60	30,34		ENE	2	Cloudy.
	2	0	57	62	30,32		ENE	2	Fair.
4	7	0	50	60	30,29		NE	1	Cloudy.
	2	0	53	61	30,27		NNE	2	Fair.
5	7	0	52	60	30,22		ESE	2	Fine.
	2	0	64	61	30,14		ESE	2	Fine.
6	7	0	54	61	30,02		ESE	1	Fine.
	2	0	68	62	29,98		ESE	1	Fine.
7	7	0	57	61	29,88		SSE	1	Fair.
	2	0	64	63	29,86		SW by S	1	Cloudy.
8	7	0	54	61	29,96	0,025	SSW	1	Fine.
	2	0	66	64	29.93		SSW	1	Fine.
9	7	0	53	62	29,91		SSW	1	Cloudy.
	2	0	64	64	29,86		SW	2	Fair.
10	7	0	53	61	29 93	0,080	WSW	2	Fine.
	2	0	62	63	29,98		W	1	Fine.
11	7	0	49	60	30,18		WNW	1	Cloudy.
	2	0	62	62	30,17		WSW	1	Fair.
12	7	0	55	60	30,27		WSW	1	Fair.
	2	0	64	62	30,26		SSE	1	Fair.
13	7	0	52	60	30,31		E by S	1	Fine.
	2	0	59	62	30,28		ESE	1	Fine.
14	7	0	52	60	30,18		NE	1	Hazy.
	2	0	56	59	30,16		E	2	Cloudy.
15	7	0	52	59	30,12		ENE	2	Fair.
	2	0	58	60	30,05		ENE	2	Cloudy.
16	7	0	50	59	29.85	0,030	E	2	Rainy.
	2	0	61	60	29,92		E	2	Fine.

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for May 1788.

1788	Time. H.	M.	Therm. without Deg.	Therm. within Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
May 17	7	0	54	60	29,96	0,031	E	2	Fair.
	2	0	68	61	29,91		ESE	2	Fair.
18	7	0	55	59	29,87		E	1	Cloudy.
	2	0	59	61	29,87				Rainy.
19	7	0	54	60	29,89	0,260	NE	1	Cloudy.
	2	0	61	60	29,93		NNE	1	Cloudy.
20	7	0	55	60	30,13		NNE	1	Fine.
	2	0	70	61	30,15		N	1	Fine.
21	7	0	55	60	30,28		NW	1	Fine.
	2	0	70	61	30,28		WSW	1	Fine.
22	7	0	56	61	30,28		SW	1	Fine.
	2	0	68	63	30,27		WSW	1	Fine.
23	7	0	57	63	30,10		SW	1	Cloudy.
	2	0	68	63	30,16		SW	1	Cloudy.
24	7	0	57	63	30,08		SW	1	Fine.
	2	0	75	66	30,02		SSW	1	Fine.
25	7	0	63	65	30,00		S	1	Fine.
	2	0	75	69	30,02		SSW	2	Fine.
26	7	0	63	67	30,00		SE by S	1	Fine.
	2	0	73	72	30,00		SSE	1	Fine.
27	7	0	63	70	29,91		SE	1	Fine.
	2	0	80	73	29,85		SE	1	Fine.
28	7	0	67	70	29,71		E	1	Fine.
	2	0	73	71	29,66		SSE	1	Hazy.
29	7	0	63	70	29,58	0,071	SW	1	Cloudy.
	2	0	61	68	29,67			0	Cloudy.
30	7	0	52	66	29,71		E	1	Cloudy.
	2	0	55	65	29,72		E	1	Cloudy.
31	7	0	52	64	29,88		NNE	1	Cloudy.
	2	0	58	63	29,94		NE	1	Cloudy.

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for June 1788.

1788	Time. H.	Time. M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
June 1	7	0	52	62	30,02		E	1	Fair.
	2	0	64	63	29,99		E	1	Cloudy.
2	7	0	54	62	29,98		SSE	1	Fine.
	2	0	68	63	29,98		SSE	1	Fine.
3	7	0	56	62	30,02		E	1	Fair.
	2	0	67	63	30,02		E	1	Fine.
4	7	0	57	63	30,00		WNW	1	Fine.
	2	0	62	63	30,04		WNW	1	Cloudy.
5	7	0	52	62	30,22			0	Fine.
	2	0	68	63	30,28		W	1	Fine.
6	7	0	60	63	30,20		W	1	Fine.
	2	0	72	66	30,08		SW	1	Fine.
7	7	0	60	65	30,00		W	1	Fair.
	2	0	72	66	29,97		W	1	Fair.
8	7	0	56	61	30,08	0,035	ESE	1	Fine.
	2	0	62	63	30,10		ESE	1	Fine.
9	7	0	52	62	30,17		ENE	2	Cloudy.
	2	0	64	63	30,19		NE	2	Fine.
10	7	0	55	62	30,21		N	2	Fine.
	2	0	69	64	30,18		ESE	2	Fine.
11	7	0	55	63	30,15		NE	1	Fine.
	2	0	67	64	30,08		NE	1	Fine.
12	7	0	54	63	30,06		NNE	1	Fine.
	2	0	70	66	30,04		NE	1	Fine.
13	7	0	57	63	30,06		NE	2	Fine.
	2	0	70	66	30,06		NNE	2	Fine.
14	7	0	58	64	30,07		NNE	1	Cloudy.
	2	0	67	65	30,06		NNE	1	Cloudy.
15	7	0	59	65	30,05		NNE	1	Fine.
	2	0	72	66	30,03		N	1	Fine.
16	7	0	59	59	30,00	0,120	N	1	Rain.
	2	0	68	66	29,96		N	1	Fine.

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for June 1788.

1788	Time. H.	Time. M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points	Winds. Str.	Weather.
June 17	7	0	60	65	29,99		N	1	Fine.
	2	0	80	67	29,95		N	1	Fine.
18	7	0	65	69	29,87		E	1	Cloudy.
	2	0	70	70	29,84		SSE	1	Cloudy.
19	7	0	63	69	29,81	0,023	SSE	1	Cloudy.
	2	0	66	68	29,87		S	1	Rain.
20	7	0	56	67	29,94	0,060	SE	1	Cloudy.
	2	0	60	67	29,94		SE	1	Rain.
21	7	0	60	66	29,98	0,030	NNE	1	Cloudy.
	2	0	60	67	29,98		N	1	Cloudy.
22	7	0	63	66	29,98		NNW	1	Fair.
	2	0	72	68	29,96		NNW	1	Fair.
23	7	0	62	66	29,80		W	1	Fine.
	2	0	65	67	29,75		SSW	1	Cloudy.
24	7	0	58	66	29,70		SSW	2	Fair.
	2	0	63	66	29,68		SSW	2	Cloudy.
25	7	0	58	66	29,63	0,245	SW	1	Fair.
	2	0	65	67	29,60		SW	1	Cloudy.
26	7	0	58	66	29,61	0,050	WSW	1	Cloudy.
	2	0	63	66	29,57		SW by S	1	Cloudy, heavy rain with thund.
27	7	0	58	65	29,53	2,116	SW by S	1	Fair.
	2	0	71	67	29,49		S	1	Fair.
28	7	0	59	66	29,54	0,285	SSE	1	Rain.
	2	0	63	66	29,54		SE	1	Rain.
29	7	0	57	65	29,73	0,311	SE	1	Cloudy.
	2	0	64	65	29,79		SE	1	Cloudy.
30	7	0	59	64	29,86		W	1	Cloudy.
	2	0	67	65	29,93		W	1	Cloudy.

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for July 1788.

1788	Time. H.	M.	Therm. without Deg.	Therm. within Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
July 1	7	0	60	64	30,07		W	2	Cloudy.
	2	0	63	64	30,07		W	2	Cloudy.
2	7	0	61	64	30,07		SW	2	Fair.
	2	0	69	65	30,07		SW	2	Fair.
3	7	0	63	65	30,04		S	1	Fair.
	2	0	73	66	29.93		SSW	1	Fine.
4	7	0	58	65	29,74		SSW	2	Rain.
	2	0	70	66	29,77		SSW	2	Fine.
5	7	0	57	65	29,77	0,040	SSW	1	Fine.
	2	0	67	66	29,73		SSW	1	Fine.
6	7	0	57	64	29,81		SW	1	Fine.
	2	0	65	65	29,89		SW	1	Fair.
7	7	0	56	64	29,85		SSW	2	Fair.
	2	0	63	65	29,84		WSW	2	Rain.
8	7	0	57	65	29,90	0,250	SW	1	Fair.
	2	0	62	65	29,80		SSW	1	Cloudy.
9	7	0	56	64	29,76	0,210	SSW	1	Fair.
	2	0	62	65	29,76		S	1	Cloudy.
10	7	0	61	65	29,81		SSW	2	Cloudy.
	2	0	70	66	29,82		SSW	2	Cloudy.
11	7	0	62	65	29,90		S	2	Cloudy.
	2	0	68	66	29,92		S	1	Cloudy.
12	7	0	64	66	29,93	0,250	S	1	Rain.
	2	0	75	67	29,77		SSW	1	Fair.
13	7	0	61	66	29,73		SSW	2	Cloudy.
	2	0	67	66	29.78		SSW	2	Cloudy.
14	7	0	58	66	29,87	0,132	SSW	1	Fair.
	2	0	64	67	29,89		S	1	Fair.
15	7	0	58	66	29,82	0,111	SSW	2	Rain.
	2	0	67	68	29,77		SSW	2	Cloudy.
16	7	0	57	66	29,75	0,082	SSW	2	Cloudy.
	2	0	64	67	29,78		SSW	2	Cloudy.

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for July 1788.

1788	Time.	Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H. M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
July 17	7 0	57	66	29,83		SW	2	Rain.
	2 0	67	67	29,85		W	1	Fair.
18	7 0	57	66	30,15	0,480	W	1	Fine.
	2 0	71	67	30,16		W	1	Fine.
19	7 0	62	66	30,14		SSW	2	Fine.
	2 0	69	68	30,08		SSW	2	Cloudy.
20	7 0	59	66	30,05		WSW	1	Cloudy.
	2 0	68	67	30,13		W	1	Cloudy.
21	7 0	60	66	30,20	0,065	ESE	1	Fair.
	2 0	67	66	30,22		NW	1	Cloudy.
22	7 0	60	66	30,21		W	1	Fair.
	2 0	71	67	30,20		W	1	Fine.
23	7 0	61	66	30,17		WNW	1	Hazy.
	2 0	71	67	30,11		W	1	Cloudy.
24	7 0	60	66	30,05		W	1	Fine.
	2 0	67	67	30,00		W	1	Cloudy,
25	7 0	60	66	30,11		W	1	Fair.
	2 0	65	67	30,10		W	1	Fair.
26	7 0	55	65	30,15		W	1	Cloudy.
	2 0	67	66	30,15		W	1	Cloudy.
27	7 0	55	65	30,14		W	1	Fine.
	2 0	69	66	30,12		W	1	Cloudy.
28	7 0	60	65	30,10		W	1	Fair.
	2 0	71	66	30,10		W	1	Fine.
29	7 0	59	65	30,11		WNW	1	Fair.
	2 0	72	66	30,10		NW	1	Fine.
30	7 0	62	66	30,16		SE by S	1	Cloudy.
	2 0	76	68	30,17		SW	1	Fine.
31	7 0	60	66	30,22		SW	1	Fine.
	2 0	77	69	30,20		SW	1	Fine.

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for August 1788.

1788	Time. H.	M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Aug. 1	7	0	65	68	30,23		SW	1	Cloudy.
	2	0	72	69	30,29		W	1	Cloudy.
2	7	0	64	68	30,45		N	1	Fine.
	2	0	73	70	30,44		NE	1	Fine.
3	7	0	65	68	30,43		E	1	Fine.
	2	0	76	70	30,43		E	1	Fine.
4	7	0	60	66	30,43		ESE	1	Hazy.
	2	0	77	71	30,37				Fine.
5	7	0	61	67	30,30		NE	1	Hazy.
	2	0	69	69	30,25		N	1	Fair.
6	7	0	57	67	30,21		NNE	1	Fair.
	2	0	64	67	30,18		N	2	Cloudy.
7	7	0	58	66	30,12		NNE	1	Cloudy.
	2	0	62	66	30,12		NNE	1	Cloudy.
8	7	0	58	66	30,19		NNE	1	Cloudy.
	2	0	62	66	30,19		NNE	1	Cloudy.
9	7	0	58	66	30,19		NE	1	Cloudy.
	2	0	66	66	30,18		ENE	1	Cloudy.
10	7	0	58	65	30,14		NE	1	Cloudy.
	2	0	66	66	30,11		NE	1	Cloudy.
11	7	0	58	65	30,08		ENE	1	Cloudy.
	2	0	68	65	30,04		ENE	1	Cloudy.
12	7	0	57	65	29,95			0	Fair.
	2	0	72	66	29,83		SSW	1	Cloudy.
13	7	0	60	66	29,58	0,325	W	1	Cloudy.
	2	0	70	67	29,51		SW	1	Cloudy.
14	7	0	58	66	29,22	0,293	SW	1	Rain.
	2	0	66	66	29,37		SW	2	Rain.
15	7	0	57	65	29,60	0,230	WSW	2	Fine.
	2	0	67	66	29,60		WSW	2	Fair.
16	7	0	57	65	29,74	0,115	WSW	2	Fine.
	2	0	57	65	29,74		SW	1	Rain.

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for August 1788.

1788	Time. H.	M.	Therm without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Aug. 17	7	0	58	65	29,72	0,673	S by W	1	Rain.
	2	0	61	65	29,64		S by W	1	Fair.
18	7	0	57	65	29,80	0,440	WSW	1	Fine.
	2	0	63	65	29,84		WSW	1	Rain.
19	7	0	59	65	29,68	0,130	SSW	2	Cloudy.
	2	0	65	66	29,66		SSW	2	Cloudy.
20	7	0	57	65	29,79	0,080	WSW	1	Fair.
	2	0	71	66	29,85		WSW	1	Fair.
21	7	0	57	66	29,99	0,023	SSW	1	Fair.
	2	0	70	66	29,98		SSW	1	Fair.
22	7	0	59	66	29,76	0,080	SSE	1	Rain.
	2	0	64	66	29,67		S	1	Rain.
23	7	0	59	66	29,67	0,045	SW	1	Cloudy.
	2	0	70	66	29,72		SW	1	Fine.
24	7	0	56	66	29,95		WSW	1	Fine.
	2	0	67	66	29,97		SW	1	Cloudy.
25	7	0	53	65	29,97	0,012	SW	1	Fine.
	2	0	69	66	29,94		SW	1	Fine.
26	7	0	57	65	29,82		SE	1	Cloudy.
	2	0	68	67	29,82		N	1	Fine.
27	7	0	57	65	29,93		W	1	Cloudy.
	2	0	63	65	29,96		W	1	Cloudy.
28	7	0	55	64	30,03		W	2	Cloudy.
	2	0	70	66	30,03		W	1	Fine.
29	7	0	58	65	29,97		SW	1	Cloudy.
	2	0	58	65	29,82		SSW	1	Rain.
30	7	0	55	64	29,82	0,230	W	2	Fine.
	2	0	67	65	29,84		W	1	Cloudy.
31	7	0	57	64	29,80	0,023	SW by S	1	Cloudy.
	2	0	62	64	29,74		SSW	1	Cloudy.

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for September 1788.

1788	Time. H.	Time. M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Winds. Str.	Weather.
Sept. 1	7	0	59	64	29,87	0,220	NW	1	Fine.
	2	0	68	65	29,91		W	1	Cloudy.
2	7	0	58	64	29,98		WSW	1	Cloudy.
	2	0	67	65	29,98		WSW	1	Cloudy.
3	7	0	59	64	30,00		SW	1	Cloudy.
	2	0	67	65	30,00		SSW	1	Fair.
4	7	0	59	65	29,75		SE	1	Fine.
	2	0	74	67	29,71		SE	2	Fine.
5	7	0	60	66	29,78		SSE	1	Fair.
	2	0	70	67	29,80		SE	1	Cloudy.
6	7	0	55	66	29,90	0,905	SW	1	Fair.
	2	0	63	66	29,94		SSW	1	Cloudy.
7	7	0	55	66	30,15		SSE	1	Fine.
	2	0	72	67	30,17		SSE	1	Fine.
8	7	0	55	66	30,14		SSW	1	Fine.
	2	0	67	67	30,10		SW	1	Cloudy.
9	7	0	57	65	30,08		WSW	1	Cloudy.
	2	0	69	66	30,08		W	1	Cloudy.
10	7	0	53	65	30,07		W	1	Foggy.
	2	0	67	67	30,07		W	1	Fine.
11	7	0	58	66	30,08		SW	1	Cloudy.
	2	0	63	66	30,16		SSE	1	Fair.
12	7	0	51	65	30,25	0,070	ESE	1	Foggy.
	2	0	67	66	30,18		ESE	1	Fine.
13	7	0	59	65	29,98		ESE	1	Fine.
	2	0	67	66	29,88		W	1	Cloudy.
14	7	0	54	65	29,94		W	1	Fine.
	2	0	62	66	30,01		NNE	1	Fine.
15	7	0	50	64	30,12		NNE	1	Fine.
	2	0	61	64	30,05		E	1	Cloudy.
16	7	0	55	63	29,82		E	1	Cloudy.
	2	0	67	64	29,79		SSE	1	Fair.

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for September 1788.

1788	Time. H.	M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Sept. 17	7	0	50	63	29,88	0,067	W	1	Fine.
	2	0	62	65	29,87		E	1	Fine.
18	7	0	56	64	29,49	0,105	ESE	2	Rain.
	2	0	62	64	29,50		E	2	Cloudy.
19	7	0	56	63	29,55	0,513	E	1	Rain.
	2	0	60	64	29,60		ESE	1	Cloudy.
20	7	0	54	63	29,57	0,280	ESE	1	Cloudy.
	2	0	60	64	29,47			0	Cloudy.
21	7	0	53	63	29,37	0,300		0	Cloudy.
	2	0	58	63	29,44		W	1	Cloudy.
22	7	0	45	60	29,55	0,128	W	1	Fine.
	2	0	53	60	29,50		SW	2	Cloudy.
23	7	0	52	61	29,75	0,145	W by S	1	Cloudy.
	2	0	60	62	29,74		SW	1	Cloudy.
24	7	0	52	61	29,67	0,302	W	1	Rain.
	2	0	58	61	29,68		SW	1	Cloudy.
25	7	0	45	59	29,81	0,062	W	1	Fine.
	2	0	59	60	29,88		WSW	1	Fair.
26	7	0	57	60	29,83		SSW	2	Cloudy.
	2	0	65	61	29,84		SSW	2	Fair.
27	7	0	52	60	29,95	0,058	SSW	2	Fine.
	2	0	61	62	30,00		SSW	2	Fine.
28	7	0	48	60	30,03		SW	1	Fine.
	2	0	60	62	29,90		SSW	2	Cloudy.
29	7	0	52	61	29,56	0,190	W	2	Fine.
	2	0	57	62	29,61		W	2	Fine.
30	7	0	50	60	29,86		W	2	Fine.
	2	0	56	60	29,92		W	2	Cloudy.

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for October 1788.

1788	Time.	Therm without	Therm within.	Barom.	Rain	Winds.		Weather.
	H. M.	Deg.	Deg.	Inches.	Inch.	Points.	Str.	
Oct. 1	7 0	53	59	29,92		W	2	Cloudy.
	2 0	58	60	29,90		W	1	Rain.
2	7 0	59	60	29,94	0,035	W	2	Cloudy.
	2 0	67	62	29,94		W	2	Cloudy.
3	7 0	57	61	29,98		W	2	Cloudy.
	2 0	63	62	30,02		W	2	Cloudy.
4	7 0	56	61	30,08		W	1	Fine.
	2 0	62	62	30,09		W	1	Cloudy.
5	7 0	56	61	30,00		SSW	1	Cloudy.
	2 0	58	62	29,95		SW	2	Cloudy.
6	7 0	55	61	29,70		SW	2	Cloudy.
	2 0	53	61	29,65		SW	2	Rain.
7	7 0	46	59	30,14	0,068	SSW	1	Rain.
	2 0	52	60	30,28		SSE	2	Fine.
8	7 0	43	58	30,55		SE by S	2	Fine.
	2 0	55	58	30,55		NE	2	Cloudy.
9	7 0	50	57	30,50		ENE	2	Fine.
	2 0	57	58	30,47		E	2	Fine.
10	7 0	47	57	30,44		E	2	Fine.
	2 0	59	59	30,44		E	2	Fine.
11	7 0	50	57	30,38		SE	2	Cloudy.
	2 0	59	61	30,35		E	2	Cloudy.
12	7 0	51	59	30,32		NE	2	Cloudy.
	2 0	59	61	30,32		NE	1	Fine.
13	7 0	51	60	30,23		NE	1	Cloudy.
	2 0	58	62	30,20		NE	1	Fine.
14	7 0	51	61	30,25		NE	1	Cloudy.
	2 0	56	62	30,22		NE	1	Cloudy.
15	7 0	42	60	30,05		NE	1	Fine.
	2 0	56	62	29,90		E	1	Fine.
16	7 0	42	60	29,66		NW	1	Foggy.
	2 0	52	60	29,64		W	1	Fair.

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for October 1788.

1788	Time. H.	M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Oct. 17	7	0	50	59	29,74		W	1	Foggy.
	2	0	56	61	29,83		W	1	Cloudy.
18	7	0	38	57	30,15		N	1	Fine.
	2	0	50	60	30,22		NNE	1	Fine.
19	7	0	33	57	30,36		NNE	1	Foggy.
	2	0	43	57	30,36		NNE	1	Hazy.
20	7	0	36	54	30,32			0	Foggy.
	2	0	50	59	30,28		SW	1	Fine.
21	7	0	50	56	30,12		SW	1	Cloudy.
	2	0	60	60	30,06		WSW	1	Cloudy.
22	7	0	49	57	30,25		WSW	1	Cloudy.
	2	0	58	60	30,30		WSW	1	Cloudy.
23	7	0	53	58	30,27		WSW	1	Cloudy.
	2	0	60	62	30,20		W	1	Fair.
24	7	0	49	58	30,12		W	1	Cloudy.
	2	0	56	60	30,10		W by N	1	Fine.
25	7	0	44	58	30,11		WNW	1	Fair.
	2	0	53	60	30,15		NNW	1	Cloudy.
26	7	0	41	58	30,19		WNW	1	Fair.
	2	0	54	60	30,15		WNW	1	Fair.
27	7	0	47	58	30,08		WNW	1	Foggy.
	2	0	53	60	30,06		WNW	1	Cloudy.
28	7	0	42	58	30,02		WNW	1	Cloudy.
	2	0	52	60	30,00		WNW	1	Cloudy.
29	7	0	41	58	30,05			0	Foggy.
	2	0	48	60	30,06			0	Foggy.
30	7	0	42	58	30,15		N	1	Hazy.
	2	0	51	59	30,22		NE	1	Cloudy.
31	7	0	45	57	30,43		N	1	Cloudy.
	2	0	52	59	30,47		E	1	Cloudy.

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for November 1788.

1788	Time. H.	M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Nov. 1	7	0	41	57	30,50		E	1	Foggy.
	2	0	51	60	30,43		E	1	Fine.
2	7	0	43	57	30,18		E	1	Foggy.
	2	0	55	61	30,08		S	1	Fine.
3	7	0	55	59	29,92		SSW	2	Cloudy.
	2	0	58	61	29,77		SSW	2	Cloudy.
4	7	0	48	60	29,61	0,190	SW	2	Fine.
	2	0	55	61	29,81		WSW	2	Fair.
5	7	0	38	58	30,13		W	1	Fair.
	2	0	47	61	30,18		W	1	Fine.
6	7	0	37	57	30,29			0	Foggy.
	2	0	48	59	30,26		SSE	1	Fine.
7	7	0	43	57	30,05		SE	2	Fair.
	2	0	49	59	29,94		SSE	2	Cloudy.
8	7	0	49	58	29,94		W	1	Cloudy.
	2	0	50	59	29,97		NW	1	Cloudy.
9	7	0	41	58	29,91		E	1	Foggy.
	2	0	53	59	29,77		SSE	1	Fair.
10	7	0	44	57	29,73		E	1	Fair.
	2	0	53	60	29,84		E	1	Fair.
11	7	0	43	59	30,05		WSW	1	Fair.
	2	0	53	59	30,12		WSW	1	Fair.
12	7	0	47	59	30,20		WSW	1	Fair.
	2	0	55	60	30,18		WSW	1	Cloudy.
13	7	0	51	59	29,85		SW	2	Cloudy.
	2	0	54	60	29,74		SSW	1	Cloudy.
14	7	0	36	58	30,08	0,200	W	1	Fine.
	2	0	42	59	30,10		WNW	1	Fine.
15	7	0	35	56	30,01	0,120	WNW	1	Cloudy.
	2	0	40	58	30,11		NW	1	Fine.
16	7	0	32	54	30,43		NNW	1	Fine.
	2	0	38	57	30,42		N	1	Fine.

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for November 1788.

1788	Time. H	Time. M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points	Winds. Str.	Weather.
Nov. 17	7	0	42	54	30,17		WNW	1	Fair.
	2	0	48	57	30,17		WNW	1	Fine.
18	7	0	38	54	30,17		WNW	1	Fine.
	2	0	45	57	30,22		WNW	1	Fine.
19	7	0	42	55	30,21				Foggy.
	2	0	48	57	30,13		WNW	1	Cloudy.
20	7	0	42	54	30,20				Foggy.
	2	0	45	57	30,20				Foggy.
21	7	0	45	54	30,26				Foggy.
	2	0	49	57	30,25		W	1	Cloudy.
22	7	0	47	56	30,25		SW	1	Cloudy.
	2	0	49	58	30,25		SSW	1	Cloudy.
23	7	0	39	55	30,20		ESE	1	Fine.
	2	0	47	58	30,15		ESE	1	Fine.
24	7	0	36	55	30,11		E	1	Fine.
	2	0	43	58	30,15		E	1	Fine.
25	7	0	38	54	30,22		E	1	Cloudy.
	2	0	38	58	30,28		ENE	1	Fine.
26	7	0	29	52	30,34		ENE	1	Fine.
	2	0	35	54	30,29		ENE	1	Cloudy.
27	7	0	28	50	30,11		ENE	1	Cloudy.
	2	0	27	52	30,05		ENE	1	Cloudy.
28	7	0	27	49	29,96				Cloudy.
	2	0	31	50	29,96				Cloudy.
29	7	0	30	47	30,01		ESE	1	Cloudy.
	2	0	34	49	30,04		ESE	1	Cloudy.
30	7	0	35	48	30,18		N by W	1	Cloudy.
	2	0	35	49	30,20		NE	1	Cloudy.

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1788	Time. H.	M.	Therm. without Deg.	Therm. within. Deg.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Dec. 1	8	o	34	48	30,22		NE	1	Cloudy.
	2	o	36	49	30,20		ENE	1	Fine.
2	8	o	31	47	29,93		ENE	1	Fine.
	2	o	35	49	29,85		ENE	1	Fine.
3	8	o	24	46	29,68		W	1	Cloudy.
	2	o	31	48	29,61		W	1	Fair.
4	8	o	24	44	29,59		N	1	Fine.
	2	o	32	49	29,58		N	1	Fine.
5	8	o	34	45	29,68		NE	1	Cloudy.
	2	o	35	49	29,75		ENE	1	Cloudy.
6	8	o	33	45	29,84		NE	2	Cloudy.
	2	o	34	48	29,83		NE	2	Cloudy.
7	8	o	34	45	29,89		ENE	2	Cloudy.
	2	o	35	46	29,93		ENE	2	Cloudy.
8	8	o	33	44	30,00		ENE	1	Cloudy.
	2	o	38	48	30,00		E	1	Fine.
9	8	o	33	45	29,98		N	1	Fair.
	2	o	39	49	29,95		N	1	Fine.
10	8	o	34	47	29,86		N	1	Cloudy.
	2	o	40	51	29,88		ENE	1	Fine.
11	8	o	32	47	30,04				Foggy.
	2	o	35	50	30,07		WNW	1	Cloudy.
12	8	o	29	46	30,10		WNW	1	Cloudy.
	2	o	32	49	30,07		WNW	1	Fair.
13	8	o	27	45	29,96				Foggy.
	2	o	29	47	29,85		NE	1	Cloudy.
14	8	o	26,5	44	29,55		NE	1	Fair.
	2	o	30	46	29,50		NE	1	Cloudy.
15	8	o	22	42	29,61		NE	2	Cloudy.
	2	o	24	45	29,63		NE	2	Cloudy.
16	8	o	22	40	29,65		ENE	2	Fair.
	2	o	27	44	29,68		ENE	2	Fine.

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1788	Time. H.	Time. M.	Therm. without Deg.	Therm. within Deg.	Barom. Inches.	Rain. inch.	Winds. Points.	Winds. Str.	Weather.
Dec. 17	8	0	26	41	30,00		ENE	2	Cloudy.
	2	0	28	44	30,08		ENE	1	Snow.
18	8	0	18	40	30,14		WNW	1	Cloudy.
	2	0	28	44	30,00		WNW	1	Cloudy.
19	8	0	26	41	29,90		NNW	1	Fine.
	2	0	32	44	29,94		NNW	1	Cloudy.
20	8	0	30	42	29,98		WNW	1	Cloudy.
	2	0	33	45	29,95		WNW	1	Fine.
21	8	0	36	42	29,66		NW	2	Cloudy.
	2	0	36	44	29,68		NW	2	Cloudy.
22	8	0	29,5	42	29,97		WNW	2	Fine.
	2	0	35	46	30,03		WNW	2	Fine.
23	8	0	23	42	30,22		WNW	1	Foggy.
	2	0	25	46	30,30		WNW	1	Foggy.
24	8	0	39	43	29,96		W	2	Cloudy.
	2	0	43	47	29,96		W	1	Cloudy.
25	8	0	41	45	29,94		W	1	Fair.
	2	0	46	48	29,80		W	2	Fine.
26	8	0	31	45	29,67		NNE	2	Cloudy.
	2	0	34	48	29,90		NNE	2	Fine.
27	8	0	30	45	29 97		NNW	2	Cloudy.
	2	0	32	47	29,90		NNW	2	Cloudy.
28	8	0	23	43	30,15		NNE	1	Fine.
	2	0	26	45	30,22		NNE	1	Fine.
29	8	0	19	41	30,30		NE	1	Fine.
	2	0	26	45	30,26		NE	1	Fine.
30	8	0	18	40	30,33		E	1	Fine.
	2	0	21	44	30,31		SE	1	Fine.
31	8	0	26	39	30 03		SSW	2	Cloudy.
	2	0	30	42	29,80		SSW	2	Snow.

1788	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height.	Mean height.	Greateſt height.	Leaſt height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Inches.
January	48	26	39,7	56	49	52,7	30,70	28,89	29,97	0,439
February	50	29	41,3	56	50	52,7	30,21	28,65	29,68	1,461
March	59	28	40,8	59	46	50,9	30,08	29,32	29,68	0,386
April	68	40	52,6	63	55	51,8	30,48	29,50	30,07	0,607
May	80	49	60,0	73	59	62,8	30,34	29,58	30,04	0,497
June	80	52	62,3	70	61	64,1	30,22	29,49	29,94	3,275
July	77	55	63,7	69	64	65,9	30,22	29,73	29,99	1,620
Auguſt	77	53	63,4	71	64	66,0	30,45	29,22	29,95	2,699
September	74	45	58,6	67	59	63,7	30,25	29,37	29,86	3,345
October	67	33	51,4	62	54	59,4	30,55	29,64	30,32	0,103
November	58	27	42,9	61	47	56,4	30,50	29,61	30,11	0,510
December	46	18	30,9	51	39	45,2	30,33	29,50	29,92	0,000
Whole year			50,6			57,6			29,96	14,892

END OF PART I. OF VOL. LXXIX.